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# ASPECTS OF TWIN RESEARCH

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## BEGINNINGS OF TWIN RESEARCH

SINCE time immemorial twins have been objects of intense interest. Human attitudes toward twins have been greatly influenced by changes in the social evolution of mankind.

Because twins and multiple births are rather unusual they were at first regarded as in some way abnormal and omens of good or evil to the families or tribes into which they were born. Even to-day some primitive tribes treat twins as either objects of awe and respect or as visitations of evil spirits, to be gotten rid of as soon as possible.

Gradually superstitious attitudes toward multiple human births have given way to scientific curiosity about them, but it was a long time before twins and multiple births became the objects of scientific study.

Prior to the twentieth century chief interest in twins was centered on the problem of the nature and origin of double monsters. A long controversy raged over this problem, in which many of the leading biologists of the eighteenth and nineteenth centuries took part. One school of thought upheld the preformation view, according to which double monsters were preformed in the egg or sperm; while their opponents, the epigenesists, believed that doubling was the result of something that occurred during the course of development. Even to-day some biologists hold that double monsters are the products of the fusion of originally separate embyros, while others regard them as the products of the incomplete fission of an originally single embyro.

It was not until 1904 that double monsters came to be regarded as instances of incomplete one-egg twinning, when H. H. Wilder elaborated this view in his significant paper, "Duplicate Twins and Double Monsters." Wilder showed that separate one-egg twins and symmetrical double monsters, for which he invented the term "cosmobia" belong to the same series. Wilder seems to have been the first worker to make detailed comparisons between the members of one-egg twin pairs and to carry this comparison to such minutiae as finger prints and palm prints.

So far as we have been able to ascertain, the first to recognize the existence of two kinds of twins, one-egg and two-egg twins, was Sir Francis Galton. By a stroke of genius he guessed that these two types of twins exist and leaped to the logical conclusion that one-egg twins were genetically alike and that two-egg twins were merely siblings conceived and born together. Galton promptly realized the use to which twins could be put as materials for genetic research.

By the use of questionnaires sent out to a considerable number of twins of

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STRIKINGLY SIMILAR "IDENTICAL" TWINS

SEPARATED AT EIGHT DAYS OF AGE AND REARED IN QUITE DIFFERENT ENVIRONMENTS. IN THE UNITED STATES ABOUT ONE BIRTH IN 88 CONSISTS OF TWINS, OF WHOM ABOUT ONE FOURTH ARE ONE-EGG (IDENTICAL) TWINS AND THREE FOURTHS ARE TWO-EGG TWINS.

both types he sought answers to two main questions:

(1) Do fraternal (two-egg) twins grow more similar as the result of being subjected for years to a common environment?

(2) Do identical (one-egg) twins grow more different after being separated and living under different environmental conditions?

The questionnaires seemed to give negative answers to both questions. Fraternal twins, instead of growing more similar as the result of living together for years, persisted in exhibiting marked differences and often became increasingly unlike; while identical twins, even when one had lived for years in India and the other had stayed in England, remained as similar as ever.

On the basis of these results Galton came to the conclusion that environmental differences, such as are to be found in the same community and at the same time, produce only slight changes in the individual's physical and mental traits.

which are, therefore, determined chiefly by "inborn nature." These first studies by Galton were published in a paper entitled, "The History of Twins as a Criterion of the Relative Powers of Nature and Nurture." Thus began the scientific study of twins.

Twin research between 1876, the date of Galton's publication, and 1904, when Wilder aroused new interest in twinning problems, was largely limited to statistical studies.

As early as 1885 Veit had made an extensive study of the frequency of twin, triplet and quadruplet births in Germany and had determined for that country that on the average there occurred one twin birth in 85 births. Ten years later Hellin announced his remarkable rule, called "Hellin's Law." This rule is that if the frequency of twins to total births is 1 to 85, the frequency of triplets is 1 to $85^2$, while that of quadruplets is 1 to $85^3$. This rule agrees remarkably closely with actual mass data. Just why, nobody knows.

In 1902 Weinberg, on the basis of the sex distribution of very large numbers of twins, came to the conclusion that not only are one-egg twins a reality, but that about one fourth of all twins are one-egg twins. Weinberg reasoned that there should be equal numbers of same-sexed and opposite-sexed two-egg twins. By doubling the number of boy-girl twin pairs (surely two-egg twins), making an allowance for the slightly greater number of males than females, and subtracting this number from the total number of twin pairs in a country, he obtained the number of one-egg pairs. This came to be known as the Weinberg Differential Method. In 1907 Nichols used the Weinberg method in determining the percentage of one-egg twins to all twins in the United States to be about 26. This was in close agreement with Weinberg's figure for Germany.

Up to the time when Weinberg showed statistically that a large number of same-sexed pairs of human twins must be one-egg twins the existence of such twins was

CLOSELY SIMILAR "IDENTICAL" TWINS

ALTHOUGH THE ONE ON THE LEFT WAS REARED IN LONDON AND THE OTHER IN A SMALL TOWN IN ONTARIO, CANADA, THEIR RESEMBLANCE IS UNMISTAKABLE. (JOURNAL OF HEREDITY)

purely hypothetical. Galton, on the basis of the very strong intra-pair resemblance in so-called "identical twins" had reasoned that such twins must have a common genetic make-up and therefore must have been derived from a single egg. At that time there was no direct embryological evidence that one-egg twinning ever takes place among mammals.

IDENTICAL ONE-EGG QUADRUPLETS
ONLY ONE BIRTH IN 600,000 CONSISTS OF QUADRUPLETS; A FRACTION OF THEM ARE OF ONE-EGG ORIGIN.

### The Turning Point

So long as the actual existence of one-egg twins remained hypothetical, little use could be made of twins in the investigation of human problems. One crucial step in the direction of laying a secure foundation for twin research was the discovery and study of one-egg twinning in two species of armadillo, *Dasypus novemcinctus* (Newman and Patterson) and *Dasypus hybridus* (Fernandez). The first reports on these two studies were both published in 1909. In both these species of armadillo but one egg is involved in each pregnancy, as is evidenced by a single corpus luteum. All fetuses in a set are of the same sex. In the Texas nine-banded armadillo (*D. novemcinctus*) four fetuses are arranged very regularly within a single chorion, two on one side and two on the other. In the South American species (*D. hybridus*) from seven to twelve embryos are derived from a single egg. In succeeding years Newman and Patterson studied and reported on the complete embryonic history, showing conclusively that the embryo up to a fairly advanced stage develops as a single individual and then undergoes two twinning divisions. The first division gives rise to symmetri-

FILIPINO "SIAMESE" TWIN BOYS AND THEIR "IDENTICAL" TWIN WIVES

cally placed twin primordia, each of which divides evenly again to form a secondary pair.

Newman made a statistical study of the degrees of resemblance and difference with respect to numbers of scutes in the


THE HAND OF MARIE DIONNE

THE HILTON "SIAMESE" TWIN GIRLS

armor and found that the intra-set co-efficient of correlation for the banded region was about .92, while that between members of secondary pairs was somewhat higher, about .935. These correlations were of the same order as those previously determined for right and left sides of single organisms.

Newman also showed that rare band irregularities in the armor (regionally doubled bands) were inherited, and in a very peculiar way. If the mother has a partly doubled band near the left margin of Band 1, fetus 1 may have a similar character on the left side, fetus 2 on the right side, fetus 3 on both sides and fetus 4 on neither side. Thus four individuals genetically alike may express an inherited character in several different ways, or even not at all. There is thus a distinct contrast between inheriting and expressing a character. There was also noted a considerable amount of mirror imaging between individuals of the same set of young armadillos.

It was only natural to suspect that, since one-egg twinning takes place so regularly in certain mammals, it probably takes place sporadically in much the same fashion in human embryos. It has not been possible, for obvious reasons, to study the early stages of one-egg twinning in man; so for the present and until we secure evidences to the contrary, we must take the twinning picture revealed by the armadillos as the best model for human one-egg twinning.

It was with this idea in mind that in 1917 I wrote a little book, "The Biology of Twins," in which I described the course of twinning in various armadillos, dealt with the extent to which variation occurs among individuals alike genetically, and then reviewed our knowledge of twinning, both one-egg and two-egg, in other mammals, including man. This little book, according to leading twin specialists in various parts of the world, served the purpose of focusing their attention upon human twins and their

possible uses as materials for research. From this small beginning has grown a new and thriving branch of biological research engaging the attention of many investigators in a dozen different countries and in a score of different fields of scientific inquiry. In 1923 I published a second book, "The Physiology of Twinning," in which were brought together for the first time all previously published data on one-egg twinning in the animal kingdom. In this book I discussed the causes and consequences of one-egg twinning and suggested the uses to which twins could be put in further research problems. Some of the theories of twinning proposed at that time excited considerable controversy and stimulated additional research.

## The Modern Period

Arbitrarily, I would be inclined to set the date of the beginning of the modern period of twin research at about 1924. Certainly the vast majority of publications in this field have appeared since that date. About that time extensive programs of work on a great variety of twin problems were begun in several countries, the chief of which were the United States, England, Germany, Austria, Japan, Norway, Sweden, Denmark, Holland, Russia and Finland.

Modern twin research may be conveniently divided into two main categories: *a*, studies of the various aspects of twins and multiple births that contribute to our knowledge of the nature, causes and frequencies of twins, etc., and *b*, studies in which twins are used as materials for the investigation of problems in a multiplicity of other fields.

Among the investigations dealing with twins themselves are the following:

"IDENTICAL" TRIPLETS

ONLY ABOUT ONE BIRTH IN 8,000 CONSISTS OF TRIPLETS, ONLY A FEW OF WHOM SURVIVE THE HAZARDS OF MULTIPLE BIRTH AND ONLY A PART OF WHOM ARE DERIVED FROM ONE EGG.

(1) Studies of the relative frequencies of twins of the two main kinds among different races and under different environmental conditions.

(2) Studies of the varying proportions of one-egg and two-egg twins in various populations.

(3) Studies of methods of experimental induction of twins in lower animals.

(4) Detailed studies of multiple births, such as quadruplets and quintuplets.

(5) Methods of distinguishing between one-egg and two-egg twins. The same

methods are also applied to multiple births.

(6) Analysis of reversed asymmetry, or mirror imaging, in twins and its significance.

(7) Studies of resemblances and differences in one-egg twins, triplets, quadruplets, and quintuplets, especially of the Dionne quintuplets.

(8) Studies of mutual intimacy and social interrelations of twins.

DEMONSTRATION OF HEREDITY
IDENTICAL TWIN SISTERS AND THEIR IDENTICAL TWIN NEPHEWS.

(9) Investigations of the prenatal and birth hazards of twins and their relatively high infantile death-rate.

(10) The reasons for the higher prenatal death-rate of one-egg than two-egg twins.

(11) Studies of the dermatoglyphics (palm, finger and sole prints) of twins and multiple sets.

(12) Studies of the inheritance of the twinning tendency.

(13) Special studies of conjoined twins, especially dealing with mirror imaging and differences between the two components.

(14) Studies of the relatively high frequency of one-egg twins in tubal pregnancies.

Among the applied aspects of twin research are those numerous studies in which twins are used to throw light on various problems in fields remote from that of twinning as such. Taking as a starting point the fact that one-egg twins are identical genetically and two-egg twins differ to the same extent as siblings, it is possible to determine by appropriate methods the extent to which various human traits are hereditary and the extent to which such hereditary traits are or may be modified by differences in environment and training.

Three main methods have been used in these studies: *a*, the concordance-discordance method; *b*, the co-twin control method; and *c*, statistical methods.

### The Concordance-Discordance Method

H. W. Siemens, a Dutch dermatologist, seems to deserve credit for the first attempt to use twins in the scientific study of pathology. In 1924 he published his book, "Die Zwillings Pathologie," in which he presented his investigations of dermatological anomalies and diseases in numerous pairs of twins and a few sets of triplets. He also is the first to have published a method of distinguishing between one-egg and two-egg twins, a method now called the "similarity method" and adopted in various modified forms by all students of twins. Siemens may be regarded as perhaps the pioneer of modern applied twin research.

The underlying concept of Siemens was that all anomalies and diseases that were always present in both members of a pair of one-egg twins and only rarely so in two-egg twins, were fully hereditary; that when such conditions were not always present in both of one-egg twins,

but more frequently so than in two-egg twins, these are partly hereditary but due partly to differences in environment. Cases of complete correspondence in both members of a pair were termed concordant (++), cases of differences in degree of correspondence were designated (+[+]) and total lack of correspondence was called discordance (+−). Siemens' methods have been extensively used in many types of study, notably those on criminality, psychiatry, ophthalmology, genetics of anomalies and diseases, etc. Such studies are too numerous to review in this brief historical sketch. Suffice it to say that through the use of this method our knowledge of the genetic basis of a large number of human characters has been discovered or confirmed, that many problems in human physiology and behavior have been illuminated, that new knowledge as to hereditary differences in susceptibility to contagious diseases has been obtained, that differences in sleeping and waking habits have proven to be largely hereditary, that criminalistic tendencies have been shown in many cases to have a definite hereditary basis, and that various well-known types of psychoses and neuroses are also hereditary.

GLADYS AND HELEN
SEPARATED AT EIGHTEEN MONTHS TO MEET AGAIN AT AGE TWENTY-EIGHT, WERE REUNITED AT THE CHICAGO EXPOSITION IN 1933. THEY VISITED THE GROUNDS IN COMPANY WITH EDWIN AND FRED, TWO "IDENTICAL" TWIN YOUNG MEN, THE FOUR AROUSING MUCH INTEREST AT THE FAIR.

KENNETH AND JERRY, ONE-EGG TWINS
BROUGHT TOGETHER BECAUSE A TEACHER VISITING ANOTHER TEACHER IN A DISTANT CITY NOTICED A BOY IN ONE OF THE CLASSES WHO STRIKINGLY RESEMBLED A BOY IN HER OWN SCHOOL.

### The Co-twin Control Method

In these studies one-egg twins must be used exclusively; otherwise the control aspect of the study is wanting. Assuming that a pair of one-egg twins is genetically identical, one member can be used as a control and the other subjected to experimental treatment. Arnold Gesell, Yale child psychologist, seems to have been the first to use this method in the study of the effects of training in young infants. As the result of the detailed study of one pair of one-egg twin babies,

he concluded that the training of one twin at a later period for a shorter time was as effective as training the other twin at an earlier period for a much longer time, and hence that much of the improvement of skills was the result of pure maturation of the nervous system, which becomes more efficient merely as the result of development.

One other alleged co-twin control experiment has been extensively publicized, namely, the case of Myrtle McGraw's Johnnie and Jimmie. This would have been a highly significant experiment had not the twins turned out to be of the fraternal (two-egg) variety and thus lacking all elements of co-twin control.

Much more extensive use of the co-twin control method was made in Russia. A group of scientists at the Maxim Gorky Medico-genetical Institute of Moscow, as part of an extensive twin research program, maintained a dormitory for a number of pairs of preschool-age one-egg twins. These were kept in an environment as nearly uniform as that usually maintained in an experimental animal colony. Many kinds of experiments were conducted with this twin colony, involving both physiological and psychological problems. It would be beyond the scope of the present article to report the results obtained and the conclusions reached, but suffice it to say that they were important. Just when this fruitful and highly promising program of twin research was in full flower it was uprooted and abandoned by the governmental authorities. And this, for reasons best left undiscussed. Now that the Russian experiments are, for the time being at least, abandoned, American investigators should by all means enter this field of research, for the method of co-twin control cries out for exploitation.

### Statistical Methods

There is not one but many statistical

THE TWO PAUL HEROLDS

EACH YOUNG MAN WAS IGNORANT OF THE OTHER'S EXISTENCE FOR TWENTY-THREE YEARS AND WAS REARED IN ENTIRELY DIFFERENT SURROUNDINGS THAN HIS BROTHER.

THE ATTRACTIVE BAILEY TWINS
ARE AS MUCH ALIKE AS TWO PEAS IN A POD OR AS TWO PEACHES ON A TREE.

methods of employing twins in genetic research. All of them, however, depend upon our ability to distinguish one-egg from two-egg twins. Much attention has been given in recent years to attempts to find accurate, objective methods of diagnosing one-egg twins. In actual practice, experienced students of twins seem to have but little difficulty, if care and a sufficiently large number of characters are employed, in diagnosing twin types.

Once having a sufficiently large group of one-egg twins, these can then be used as a basis for determining the extent to which hereditary characters may vary in expression either under nearly identical environments or under environments differing in all sorts of ways.

A considerable number of studies based upon a comparison of the relative intrapair differences between one-egg and two-egg twins have been conducted by both American and European workers. The pioneer study in this field was made in 1905 by E. L. Thorndike, dean of American educational psychologists, but his work was considerably hurt by his failure to be able to diagnose twins of the two types, and by his decision, which he now knows to be ill-founded, that all twins belong to a continuous series and have a common mode of origin. Following Thorndike's lead, Lauterbach, Merriman, Wingfield and Hirsch all attacked the hereditary-environment problem and approached a more satisfactory analysis of it.

The most recent study in this field is that of Newman, Freeman and Holzinger. This study published in 1937 in book form is in most respects perhaps somewhat more exhaustive than preceding studies and may be taken as an example of this type of twin research. One-egg

ELEANOR AND GEORGIANA

WHO, AFTER TWENTY YEARS' SEPARATION, WERE BROUGHT TOGETHER AS A CONSEQUENCE OF THE FACT THAT A CATHOLIC SISTER WHILE TRAVELING ON A BUS MISTOOK ONE FOR THE OTHER.

twins (50 pairs) are used as a control. For each pair the heredity is the same and the environment as nearly alike as humanly possible without laboratory control. One of course assumes that any differences between one-egg twins must be due to differences in either prenatal or postnatal environment or to both. If now we take an equally large group of same-sexed two-egg twins and find, as we

do, that the intrapair differences are much greater in almost every character than those of one-egg twins, we can calculate the extent to which hereditary factors alone are responsible for differences in two-egg twins. The method developed by my colleague, K. J. Holzinger, is to subtract the intrapair coefficient of correlation of two-egg twins from that of one-egg twins and divide the remainder by 1 minus the intrapair coefficient of correlation of two-egg twins. This gives the percentage share of genetic differences responsible for intrapair variance for one character at a time. The formula used was: $h_2 = \frac{{}_i\mathrm{r} - {}_f\mathrm{r}}{1 - {}_f\mathrm{r}}$, where $h_2$ is the percentage share of hereditary determination of the observed intrapair difference in two-egg twins, ${}_i\mathrm{r}$ the intrapair coefficient correlation of identical (one-egg) pairs and ${}_f\mathrm{r}$ that of fraternal (two-egg) twins. Thus for any character measured for both groups of twins the shares of hereditary and environmental factors responsible for observed differences may be determined with a high degree of accuracy, limited only by the accuracy of measurements and tests used.

Another phase of our work was the rather laborious collection of twenty pairs of one-egg twins separated in infancy and reared apart under a great variety of different environmental conditions. In this study it was found that the intrapair differences with respect to nearly all physical characters were no greater than those in one-egg twins reared together, indicating that any significant differences in these characters are probably determined by differences in prenatal environment.

CECILE, MARIE, EMILIE, ANNETTE AND YVONNE DIONNE
AT ABOUT TWO YEARS OF AGE, THE ONLY KNOWN LIVING QUINTUPLETS; THEY ARE "IDENTICAL."

Intrapair differences in mental ability and scholastic achievement were, however, considerably greater in the separated than in unseparated pairs and these differences were definitely corre-

THE BADGER QUADRUPLETS
THE TWO ON THE LEFT HAVING BEEN DERIVED FROM ONE EGG AND EACH OF THE TWO ON THE RIGHT FROM A DIFFERENT EGG. CONSEQUENTLY THE SET WAS DERIVED FROM THREE EGGS.

lated with degrees of difference in education. Temperament-emotional differences were also in many cases quite marked in the separated pairs and could usually be referred to marked differences in environment and experience. The individual case studies of these twenty pairs of separated one-egg twins often revealed the ways in which differences in environment had a moulding effect upon personality traits. It would hardly be appropriate in this general review of twin research to discuss our own contributions at greater length.

The field of twin research is a rapidly expanding one. Already my own private bibliography contains titles of over five hundred publications about twins and multiple births. There are still many unsolved problems and open questions in this field that would well repay further research.

# EXPLORATION OF MUMMY CAVES IN THE ALEUTIAN ISLANDS

## PART II. FURTHER EXPLORATION

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### AMLIA

FROM the Atka natives we have learned of a "mummy cave" on the long island to the east, Amlia; and on July 4, at 4 A. M., start for the same, with a native guide, former Atka chief, on the *Talapoosa*. By 8 arrive opposite a cove on the northern shore. The "cave" is on the south shore, but the sea is rather rough, barometer unpromising, and Captain unwilling to risk under such conditions dangerous south coast. So dory takes party to the cove we see, and at 9 A. M. we start on a walk across. The native says it is "a 2 hours' walk." Climb over hills, lowlands and along a lake, partly in water, 3¼ hours, before reaching the place. The native knows of the cave only "in general," but has guided us well. On a gravelly isthmus we find a moderate-sized old village site, and to the S. of this among great fallen rocks two skulls. Further on along the cliffs discover two rock shelters, with several more skulls and some bones, also a wooden dish over a skull, remnants of a red kayak and few other objects, but no cave, no mummies. A small platform built rather ingeniously in a rock-cleft above one of the shelters did probably once hold a mummy, but nothing is left of it—the trappers had been here first.

Cold, drizzly; fire in a native little half-underground lean-to nearly chokes at first with smoke, but we manage to make a can of coffee. Explore cliffs and rocks on all sides—no other remains discoverable. Dig a little—2 stone implements.

In the rock shelters get two pieces of fine wood-carving, and some bone implements.

Have to depart at 4—reach north cove 7:15. Dory there waiting—a rough trip to ship—have to climb up on emergency matting—but all ends well. At 9:00 P. M. start westward, for excavations on unknown Agatu, one of the two westernmost Aleutians.

The party on this trip visited Adak, Attu, the Commander Islands, and was then landed and left alone on Agatu, where for three fruitful weeks excavations were carried on in two old sites. But the caves and especially the Shiprock were not forgotten. On August 8 the party was taken on board the fine new Cutter *Duane*, Captain P. F. Roach, and proceeded eastward. The first stop was to be made at Tanaga.

### TANAGA, ILAK

Off the large island of "Tanaga," Makarii, the old chief of Atka and our guide at Amlia, saw "many years ago" a mummy cave or shelter. The information as to the location of the cave was rather indefinite and without any details as to contents, but in the main probably reliable, and so it was decided to look for the hollow. This is how we fared:

*Aug. 10, 1937.* Fog. At 1 P. M. reach off Tanaga—but at first the wrong bay. Fight fog and current rest of afternoon, without success. Anchor for night somewhere off the coast.

*Aug. 11.* Higher winds; less fog, but can see only a small part of land on left. Ship rocks more, though not bad. Prospects not good.

9 A. M. Fog thickens again, no land in sight any more. Ship about five miles off shore, but dares not send a boat out in the fog.

10 A. M. A dory sent out to reconnoiter, under protection of ship's sirens—returns in half an hour without having seen anything—could not dare to go further. Captain very nice about it all, will wait.

11 A. M. Fog all around, visibility only about 200 yards in any direction.

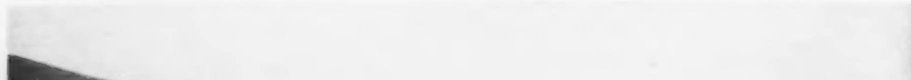
12 to 6 P. M. Dory sent out twice—could not prevail, once lost for a time. Fog the same. At 4 sun partly penetrated, but did not show itself full, nor dispelled.

9 P. M. No change.

*Aug. 12,* 6–7:30 A. M. At last sea nearly free from fog. Ship has been riding far out, now returning once more towards land. A cape dimly visible towards the west. Land still in haze, but perceptible. Sea somewhat rough.

9–1 A. M. Have reached near pinnacles and rocks off land, visibility now fairly good. Leave in the dory for a small "black island" somewhat over a mile off. Circle around, find it volcanic, craggy, unboardable even on the lee side, and wholly unpromising—not high enough, and no caves or overhangs where water could not reach them.

Pass on to two small islands in the western end of the "bay," about three miles from the first. The one to right, nearer land, partly grassy, partly craggy, unpropitious. The further one,



THE "GANG" OF VOLUNTEER STUDENTS

ACCOMPANYING THE AUTHOR IN 1937. THE TOOLS OF THIS PARTY ARE THE PICK, THE MATTOCK, THE SHOVEL AND THE WHEELBARROW. THE DRESS CONSISTS OF RAINCOATS AND HATS, WARM UNDER AND OUTER CLOTHING, HIGH BOOTS AND CANVAS GLOVES. LEFT TO RIGHT: WALTER WINEMAN, PAUL GEBHART, PAUL GUGENHEIM, SYDNEY CONNOR, ALAN MAY, STANLEY SEASHORE.

DISEMBARKING OF THE AUTHOR

WHO, NOT HAVING HIS HIP BOOTS, MUST BE CARRIED ASHORE ON THE SHOULDERS OF A SAILOR. THE BOAT AND CREW ARE FROM THE COAST GUARD BOAT WHICH BROUGHT THE PARTY OVER.

volcanic, craggy, about 60 feet high, sealed—getting out of the boat perilous—and examined—no vestige of anything human; and no cave or crevice that could have had any remains.

In distance—about five miles—towards the east or southeast, see a point and off there a rock with two islands—more likely our destination; but sea rather rough, 30 mile wind, must return to ship before attempting to reach these. Come back wet, but without any damage. It is 11 A. M. Ship now moves towards the above islets and endeavors to anchor (43 fathoms) about a mile off—but anchor will not hold, sea is rougher, and so Captain decides to try Ilak first. Have sighted it in haze to the south. The isle has a bad reputation for storms and dangerous rocks; but there was known to be in its cliffs a cave with mummies. Many years ago—Mr. Willis, now of Dutch Harbor, found this cave and saw in it, "sitting around," probably a score of mummies. But, it was learned later, three years ago, the cave had been sacked by a couple of fox trappers, who were known to have brought out a good deal of "loot"—though not the mummies—and who a year after were capsized off the place and drowned.

Reach off Ilak—16 miles from our anchorage at Tanaga. Not easy to locate a landing place for the dory. Find, most unexpectedly in this entire volcanic region, a wholly *granitic* and not volcanic island (whitish stone), with a flat top. Much like one of the southwestern mesas. Perhaps 300 feet high, grassy slopes and foreground (north), a bouldery beach, many shore and off-shore "bad" rocks. On the northwest part see high rough cliffs. A trapper's shack about 100 yards from water near middle of north shore. Bad walking in tall

grass—at times some of the party not visible in the rank vegetation. Along shore must jump like goats over boulders, yet these offer the best way. In parts the granite outcrops stand on edge, offering broken sharp ridges.

Explore everything likely towards northwest, and about a mile from the shack locate a cave, in a high bare rough pinnacle, with a lot of human debris, wood, stones, human bones, parts of four skulls. Digging shows about 2½ feet of debris—soil, stones, more or less decayed wood, grass, traces of matting, stray human and bird bones; underneath more wood, then a good layer of debris of mollusks (limpets), with some bird bones, a few chipped, and one polished, knives, a wooden bowl, no human bones; and lowest down flat fire stones, ashes, debris.

Cave had evidently originally been occupied, then used for a burial place. Not long ago the whole was disturbed, mummies and skulls taken away, bones with remains of wooden utensils scattered or thrown out. Doubtless the reported mummy cave—the driftwood poles and debris indicate that—but now all vandalized by the trappers.

We collect what is worthwhile, and excavate whole accumulation in the not very large space. Subsequently one of my boys (May) discovers another "hole in the rocks," somewhat further westward, with six mossy skulls, some in good condition. And another of the boys, a former Scout (Conner), is sent on a trip around the island—but finds no more man-used caves, or other traces of human remains.

Carry everything saved back over the

OLD SITE OF UMNAK WITH PRESENT ALEUT DWELLING

NOTE THE RACK FOR DRYING FISH AND TWO PARTLY UNDERGROUND CHAMBERS, ONE USED AS THE SWEATBATH AND THE OTHER FOR STORAGE.

HELL'S KITCHEN ON AMLIA ISLAND

AN EXAMPLE OF THE EXCESSIVE EROSION AND ROUGHNESS OF THE COAST IN MANY PARTS OF THESE ISLANDS. AN OLD BURIAL CAVE IS CLOSE BY BUT NOT VISIBLE.

rocks and through the grass, including a heavy movie camera, reaching hut quarter to eight, sweating and wearied; have some hot coffee there, made by boat men who stayed behind, with raisin bread. At dusk depart, at 9:30 back on the ship. Weather better now, wind less, but Captain tired, has gone to sleep, and so ship remains here over night.

*Aug. 13.* Wind weak, sea fairly good though a swell, horizon misty, but no real fog. Tanaga not visible.

At 9 up—anchor and back to Tanaga.

9:15—wind freshens, fog envelopes top of Ilak. Run slowly.

At 12 anchor off Tanaga again. Rather fairly clear now, sea with smooth heave only.

12:30—out with the dory to explore what seemed the two rock islands close to shore—only to find them to be but broken small promontories, connected with the main. Explore next a grassy island not examined on previous visit—find it volcanic, rough—except for an elevated mass covered with vegetation and with a flat top. Not a vestige of anything human. Before finishing fog advances from land, until ship is lost from view. Recall everybody, call the dory which had to ride outside not to be damaged by the heaving water on the rocks—and return—nothing else to do. The Tanaga cave remains undiscovered.

Next day search rocks off west coast of the large island of Adak—no caves; but on the shore an extensive old site.

Explored on this trip also rock shelters in Korabelni Bay, Atka Island—a few skulls and skeletons, but main "cave" (rock shelter) despoiled.

### Shiprock

A memorable item, however, still

awaited us. This was the visit—this time successful—to the great rock in the midst of the rough Umnak Pass, known as "Shiprock." Again it will be best to give the original notes, which preserve a flavor that can not be duplicated later.

*Aug. 19.* Reach off Shiprock at 9 A. M. Soon on a dory, and at 10:15 there. Beach all huge rocks. Find the little landing place Krukhov told about, go up steep slopes to overhanging cliffs and explore. Island larger than seems from distance, shore very rough. Luckily but little surf.

Soon find a great long rock overhang, and in it a structure of driftwood, with two skulls and some attached bones visible; and before long see it is an undisturbed deposit of mummies and burials. Excavate intensively whole day. Boat brings lunch and remaining boys. A great harvest, next to that of last year on Kagamil—and as then at practically the last day of the trip!

There were two separate shelters here with burials. In the main one, the bodies had been placed on a structure of driftwood poles and on a big portion of a whale skull. On this base lay three whale scapulae, then mummies, and over all this was an inclined roof of parallel partly dressed poles leaning against the wall; and this "roof" had once been covered with skins of sea-lions.

To the left of this main part were about five feet of later burials, males on top, females and children farther to the left and lower.

The mummies were much as those at Kagamil, but there were no children in their carriers and nothing remained in entirely whole condition. The bodies had been less well strapped, also, and there was less matting; but two of the mummies below their outer skin cover showed remnants of highly decorated skin dress and matting.

Had to send back for more sacks and burlap and cord. Quite a haul, and skulls of mummies *all of the oblong,*

MUMMY SHELTER ON SHIPROCK, UMNAK PASS

WITH DEPOSIT OF MUMMIES AT RIGHT BELOW AN OVERHANGING LEDGE. ON THE LEFT AND BELOW, AMONG THE ROCKS, OUR DORY MAY BE SEEN. APPROACH TO THE PLACE IS OFTEN IMPOSSIBLE.

*pre-Aleut, variety.* A highly important find, and undisturbed, only affected by age—though not excessively ancient. No trace of white man's influence, and no disease save arthritis.

Had to hurry—ship wanted to leave at 5—but after all stayed for us till after 6. Officers and men with us were very helpful. It is hard to give credit enough to the Coast Guard for all their aid.

Good many of the female and children's skulls on the side of the mummy structure showed the Aleut type—evidently later burials.

In second shelter, marked on surface by two posts and a nicely dressed crossbar, only regular burials, no wrappings, no objects, about six or more bodies and all Aleuts.

*Evening.* Had once more to telegraph for large barrels. Everything now stacked on deck, as last year, even though not quite so much.

As we finished, fog began to invade everything once more, and before we reached the ship with the last load the Shiprock could be seen no more.

When we reached the ship—6:30 P. M.—they were hoisting the anchor—water said to have been over 100 fathoms. This necessitated the dory's "staying off" in the now rough water for over half an hour. Tossing and rocking so much until the spine, from so much bending from side to side, felt like in a moderate attack of lumbago.

### Shiprock, 1938

Thanks, once more, to the Coast Guard, the search of main previously found burial caves could be resumed, and supplemented by that for new ones. The party on the small Cutter *Ariadne,* Captain H. W. Stinchcombe, left with the expedition June 1st, and June 2nd reached once more the Shiprock in the Umnak Pass.

*June 2, 9* A. M. Spent fair night, now on dory for Shiprock. Cloudy, but signs of clearing. Sea so far moderate.

MUMMY DEPOSIT ON SHIPROCK ISLAND
THE MUMMIES ARE LOCATED ON THE RAFTERS UNDER THE ROCK LEDGE ON THE RIGHT. SIDNEY CONNOR AND ALAN MAY, TWO MEMBERS OF THE EXPEDITION, MAY BE SEEN.

10 A. M. Have reached the rock, found everything as we left it. Excavate in farther depressions below overhang. Soon find more bones, Aleut burials. Continue till 12:30, when boat brings captain and lunch.

All skeletal remains found to-day from early Russian period—some large and small white glass beads deep among them. Most skulls and bones in poor condition, number of skulls broken to pieces. Some mixture of oblong (pre-Aleut) with rounded (Aleut) heads. The two strains have evidently intermingled on Umnak, and remnants of the larger pre-Aleut people were still here on the advent of the Russians.

With the bones found four flat stone lamps, a number of obsidian arrow points and scrapers and a few small chipped knives of other stone. One of the lamps has a handle, to the right of front, a turtle-like head and neck. An-

other, larger, is triangular. All of very moderate depth, fair to good workmanship, dark to black stone, resembling those of Kashega.

At our old rock-shelter, during lunch, found odd petroglyphs on one of the large rocks. Also red paintings—lines and curves—on various parts of the base of the whale skull that we got out of the shelter last year. Did not see these artifacts last season due to dirt on the objects.

Explore all around the rock—no caves or other rock shelters.

Start back 3 P. M. Day clear, beautiful view of the S.E. Umnak as well as the Unalaska volcanoes; but windy and water now rough, with great current. Captain, fearing accident, has us discharged on the island opposite (Umnak), and with us walks about 5 miles over beaches, rocks, tundra, with stiff wind against us. Have to cross a swift thigh-deep stream, could hardly keep upright. Walk takes over 2½ hours. Then dory picks us up, through surf, and we start on rough trip to the cutter. Before we can reach it however are caught by a "willy-wow," violent localized wind, driving cold spray at times clear over our heads. All wet. Bump into cutter and there is quite a time in getting us out and the dory on the ship; fortunately dory new, men efficient and so no bad damage. But a trip not for weaklings.

After 6, supper—not much appetite after that walk and run. Stay overnight at anchor—a gale, and sea rough.

ALEUTIAN MUMMY, FROM SHIPROCK

ALL THESE MUMMIES WERE BURIED IN THE "CONTRACTED" POSITION, DRESSED IN SKIN GARMENTS COVERED WITH MATTING OR SKINS AND TIED.

### Kagamil

*June 5.* Bright morning. Umnak NW. Volcano, Vsevidov, glorious. Finally get to inner harbor of Nikolski—was too rough yesterday. Arrange for later work—get a couple of specimens from boys—and after 9 start once more for Kagamil, to satisfy finally about hazy report of additional cave.

Reach after 1—divide party—explore coasts for over two miles to northeast and up to near vertical cliffs to west—no cave. But a small old village near trapper's hut and another in a northeast bight—neither important. Examine also rock shelter in side of hill behind and to west of hut—nothing there.

Many beautiful views during the day of the Umnak volcano, and somewhat also of Mt. Carlyle. Once saw, too, the upper part of Mt. Cleveland, with a small wreath of smoke from top. Could not revist our caves, which still attract—perhaps later in season.

*June 6,* 8 P. M. Start once more for Amlia. Sea fairly good at first, but soon

THE "GANG" LUNCHING AT THE MUMMY SHELTER ON SHIPROCK IN 1937
THE DIGESTIVE ORGANS OF PERSONS IN THESE REGIONS ARE NOT PARTICULAR, IF THERE ARE SUBSTANTIAL SANDWICHES AND PLENTY OF HOT COFFEE.

rough, large waves, ship rolled much, kicked, bumped, shivered. Whole night so, more or less, and morning worse. At breakfast everything sliding and knocking—no meal. By near 9 pretty near sick; but at 10 enter Sviechnikov's harbor in south coast of Amlia—a great relief to all; but weak, and stomach bad.

After lunch — poor lunch — mouth muddy—leave for headland. Search and climb over slippery rocks, and at last find the cave—a big orifice in base of huge basalt bluff, in a ravine. Cavity largely filled with rocks, among which traces of some mummies—could not have held much—but what there was has been despoiled. A search among and under the rocks gives two damaged female Aleut skulls, some bones and a nice large wooden dish. Search all neighborhood —nothing further.

### Kanaga

*June 9.* Heard of a burial cave on the Kanaga Island. Leave Amlia 4 A. M. Rough. Ship rolls and tosses so that sleep or meals impossible.

Afternoon fog in addition, but sea slightly better, though big ugly waves. After 5 a partial clearing, just enough to enable us to pass safely into the little Kanaga harbor. Entrance narrow, rocks both sides, wreck of the U. S. S. *Swallow* on those of the left, dismal, washed and sprayed over by angry waves. A sombre sight.

*June 10.* Gale from NW, impossible to lower dory. Men of little local Navy station know several sites—we get a number of skulls and a skeleton—but no cave. Hear of one on south coast of the island, but distant and information not definite enough.

### Ilak

*June 11.* Morning quiet; sun, nice, mild. Leave 7 A. M. for another visit to Ilak to make sure nothing was left—but stop for some time to examine the wreck of the *Swallow* and salvage some things.

Reach Ilak after lunch, explore till 6. Revisit the two caves—nothing left in them but a few stray bones. On a bluff at the southeast end, however, facing a smaller island, I find an old village site, not known of before.

In the afternoon sky gets clouded, southeast wind rises, gets stronger, and by the time we are through ship had to be moved out of the wind. A rough getting back and especially aboard; but all ends well.

To-morrow Amchitka. No possibility as to Tanaga.

### Last Visit to Kagamil

Between June 11 and August 10 the expedition excavated at Amchitka and Umnak, and surveyed for old sites on the Commander Islands. On Aug. 10, on the Cutter *Shoshone,* Captain J. Trebes, r., we reached once more the Four-Mountain Group, to have a last look at our caves.

The weather is rough and foggy. Reach the islands, but can not anchor—keep going about till morning, then anchor near foot of Mt. Cleveland (but partly visible at any time). During day a bad SW "full" gale—no possibility of doing anything—wind 60 miles an hour.

*Aug. 11.* Gale over, though still much swell. Ship goes over to Kagamil, anchors off the rough shore, and soon we reach with dory the cove where we em-

AN ALEUT COUPLE IN ALASKA

THE MAN IS WEARING A TYPICAL OUTER GARMENT WITH DECORATIONS, AND A TYPICAL DECORATED WOODEN HAT. THE DRESS OF THE WOMAN HAS ALREADY BEEN MODIFIED BY RUSSIAN INFLUENCE. THE MAN REGRETTABLY DOES NOT APPEAR TO BE AN ALEUT BUT IS RATHER A NEGRO IN ALEUT CLOTHING AND HAT. (AN OLD DESIGN BY CHORIS.)

barked the second day in 1936, and are confronted with the same old huge bad slippery boulders. Examine two miles of roughest shore, to beyond warm cave, revisit both caves, and excavate, get in recesses of rocks 5 more skulls, some bones, 2 long bone dart harpoons, a stone lamp and a few other specimens. Light rain most of day, get all wet from grass; but in warm cave fairly comfortable, and at 4 back to the ship. Officers and men from ship helped again all along, all most friendly. In the warm cave, below all the mummies removed in 1936, under a great slab—it took four of us to lift it—found an additional cremation burial of a woman and a child, doubtless sacrificed slaves. Layer of burnt bones however extended over a large space farther—below all former contents of main part of cave—and showed the calcined remains of more victims—apparently almost all females—possibly as many as ten individuals. An interesting fact was that the fire in which the bodies were burnt was not that of wood, which in these parts is non-existent, but of cancellous whale bones rich in fat. The bones of the cremated were short and rather weak, Aleut-like; but one much burnt skull in pieces, taken for reconstruction, has shown the type of the pre-Aleut people. The next day back to Umnak, where we found a whole great pre-Aleut mound.

### Scientific Results

The scientific results of our cave exploration in the Aleutian Islands can not as yet be fully appraised, for it will take long for the materials to be studied. What can be said in general, is as follows:

Notwithstanding the ravages of time,

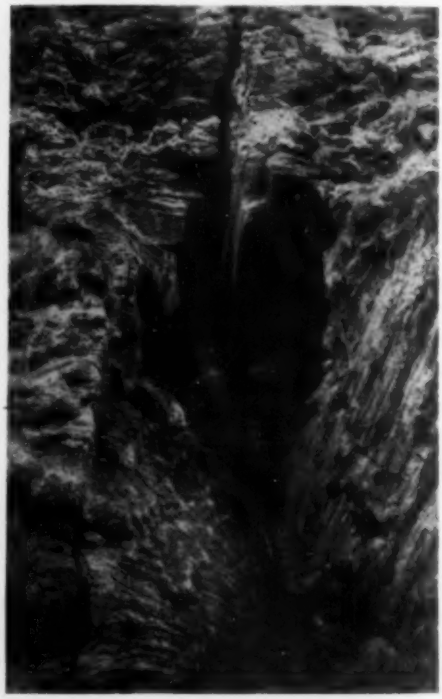
AN ALEUT PAIR, SHOWING THE TYPICAL NATIVE MALE AND FEMALE DRESS AS WELL AS THE TYPICAL ALEUT WOODEN HAT. THE FIGURE ON THE RIGHT SHOWS THE PHYSIOGNOMY OF A WHITE WOMAN—PERHAPS A RUSSIAN-ALEUT HALF-BREED. IN THE BACKGROUND THERE MAY BE SEEN TWO *baidarki*—NATIVE CANOES. (AN OLD DESIGN BY CHORIS.)

ALEUTS OF MIXED BLOOD
MAINLY RUSSIANS, ON THE ISLAND OF UMNAK. THESE PRESENT-DAY "ALEUTS" ARE A WEAKENED LOT, AND ARE SUBJECT TO MANY WHITE MAN'S DISORDERS.

and extensive vandalism by trappers and other white men, a considerable amount of the old remains, skeletal and even cultural, have been preserved and saved.

The number of skeletons and skulls from the found caves reaches several hundred. The mummies alone counted over 70. The latest of these remains are doubtless from the earlier part of the Russian period, the earliest materially antedate it; but none are older than the second millenium, and probably the second half of the second millenium, of our era. As none of the other mummies or remnants of mummies that so far were found in the Aleutian caves showed any greater age, it is inevitable to conclude that the practice in the islands was not very ancient. It is true that there may be mummy caves not yet discovered and the contents of which may throw further light on the subject. It is further quite probable that the mouths of other such caves have been sealed forever by falls of rock during earthquakes. But had the practice been ancient at least some of the caves that were found would have shown it, for they were old formations.

The mummifications and cave burials were practiced by the Aleuts up to and for some time even after the advent of the Russians. The proofs of this are the presence in a few of the latest burials in the Kagamil caves of syphilis, which was wholly absent in the mass of the earlier material; also the find in one of the caves, with superficial remains, of remnants of an old-fashioned "shoe-



THE HOT MUMMY CAVE ON KAGAMIL
APPEARS FROM THE OUTSIDE TO BE A HUGE SLIT IN THE LAVA DEPOSITS, BUT INSIDE IT ENLARGES INTO A GOOD-SIZED IRREGULAR CAVITY. TWO MEMBERS OF THE EXPEDITION MAY BE SEEN IN FRONT OF THE ENTRANCE TO THE CAVE.

ALEUTS OF MIXED BLOOD, PARTLY RUSSIAN
TWO OF THE STURDIER YOUNG MEN, WORKING IN THE SEAL INDUSTRY ON THE PRIBILOV ISLANDS.

maker's" iron-bladed knife, and also of a piece of a white man's cord.

The introduction of the methods of both mummification and cave burials was apparently due to the easily identified broad-headed Aleuts. The islands, it is now definitely established, had had for many hundreds of years before the Aleuts came, an extensive pre-Aleut population of taller, oblong-headed, more Indian-like people. It is now certain that all the older sites throughout the islands belong to the pre-Aleuts, though Aleut remains may be found in many on the top. The Aleuts, it seems, could hardly have been in the islands for

THE INHABITANTS OF ATTU VILLAGE
ON ATTU ISLAND, WESTERN-MOST OF THE ALEUTIAN GROUP. (U. S. NAVY.)

MOUNT VSEVIDOV ON UMNAK ISLAND

ONE OF THE MOST STRIKING VOLCANOES. THE TOP OF THE MOUNTAIN WAS BLOWN OFF LONG AGO. (C. & G. SURVEY, 1938.)

more than perhaps three hundred years before the advent of the Russians. The great "middens" which by Dall and Jochelson have been thought to be Aleut now appear generally with but an Aleut veneer but with the bulk pre-Aleut.

There is no indication that these sturdier pre-Aleuts perished or were massacred by the newcomers. They probably moved eastward, to the continent. They had not practiced mummification or cave burial during their stay in the islands before the advent of the Aleuts. But, especially on Umnak and the more western islands, the last pre-Aleuts who were still there when the Aleuts came, mingled and mixed with these and adopted some of their customs, including to some extent—especially at Umnak, it is evident—the new form of burial. Thus at Shiprock (Umnak Pass) the mummies—males—were pre-Aleut, while directly by them there was a mass of burials of Aleut women and children. In the Kagamil caves, while the Aleuts predominated, there were both types and some intermediates. One cremated slave in the warm cave was, it could be determined, pre-Aleut, while the others, so far as could be told, were of the Aleut type. And similar indications of a brief coexistence of the two types were given by other islands. Shortly after the advent of the Aleuts the pre-Aleut strain disappears; yet to this day a few traces of it seem to occur among the living western Aleuts.

ALEUTS OF MIXED BLOOD

THE MAN IN THE CENTER COMES NEAR TO THE OLD TYPE. THE REST HAVE MORE OR LESS WHITE BLOOD, MAINLY RUSSIAN.

From where the Aleuts brought or got the usages of mummification and cave-deposits of the bodies, is uncertain. No such habits as yet are known of in northeastern Asia; and similar procedures further to the eastward—on Kadiak, in Prince William Sound, on the islands of SE. Alaska and British Columbia—do not appear any older or even as old. The whole custom may have gradually developed among the Aleuts themselves.

FOUR-MOUNTAIN ISLANDS, LOOKING SOUTHWEST FROM ULIAGA

MT. CLEVELAND IS ON THE LEFT, MT. CARLYLE ON THE RIGHT. HERBERT ISLAND IS VISIBLE IN THE DISTANCE.

Nothing whatever of that nature is known from among the Eskimo, or the mainland Alaska Indians.

The mummy bodies without exception were in the typical contracted or "fetus-in-the-utero" position, with the limbs folded close to the body, the hands under or on the face, the head bent a little forward. The little children were in skin- or basketry-like carriers, premature births in small bundles or wooden dishes.

Specimens with the mummies were numerous, and some were of high interest. They ranged from whole kayak skeletons—paddles, war-shields, pieces of armor, garments, dart-shafts, baskets, bags and a wonderful assortment of matting, some exquisitely made and decorated, to fine labrets and spoons, and some common stone lamps and utensils. The abundance and variety of the perishable objects amounted to a veritable

MOUNT TULIK, A SLEEPING VOLCANO, ON UMNAK ISLAND
TAKEN FROM UMNAK PASS. (C. & G. SURVEY, 1938.)

resurrection of an important part of the old industries and give a radically new light on the Aleut culture.

The bodies were wrapped in sea-otter fur or bird-skin robes (parkas) and mats, the whole bundle being tied into another mat or a sea-lion skin, laced together or tied with interestingly made cords, or with ropy kelp.

The bundles so far as perceptible had not in general been suspended, but lay close in a mass or on rafters; some may possibly have been hung from the driftwood posts in the cold Kagamil cave, but no mark remained of such a suspension.

The adult mummies, some still in a very good condition, showed each a rough opening, some through the perineum, some through the upper part of the chest, through which doubtless the viscera were extracted. In no case, however, from any locality, was there any remnant of a stuffing of the body. Such stuffing, if practiced, must therefore have been limited to some locality from

which there is no representation, or has in the course of time completely disappeared.

In at least two cases, both from Kagamil, there was preserved an individual skull, once in wrappings, once in a wooden dish in moss. The latter, which alone so far was examined, is the skull of a young adult female without the mandible, lying snugly in the moss on its right side. It was evidently preserved thus already as a skull. These may have been trophies, or skulls of especially loved or esteemed individuals.

The deposits in the warm cave on Kagamil gave also an abundance of loose feathers of several varieties of birds, a good many dried wings, and even some dried whole birds. These were doubtless offerings. One wooden dish contained no less than 18 dried wings of the "pine-grosbeak," another a dried brown hawk skin. And there were a number of dishes with odds and ends of woman's work.

Among many loose skulls in the humid cold cave of Kagamil was one, normally developed, of a very extraordinary size (2005–2010 cc capacity). As there were no outstandingly large bones in the cave, this skull could not be attributed to any giant, which makes it the more remarkable.

The study and description of all the cultural material will require much application and must be left to the experts on such matters.

## Conclusions

Our expeditions in the Aleutian Islands, under the auspices of the Smithsonian Institution, have located and explored a series of mummy or burial caves and rock-shelters, which yielded collectively a large amount of both skeletal and cultural materals.

The mass of these materials are from pre-Russian Aleuts; but the latest burials in the caves or shelters were post-Russian; while among the earlier ones there was a scattering of the pre-Aleut people.

The mummies and burials included both sexes and all ages; but there was noted here and there some segregation.

The mummies in general had not been suspended, but laid one upon the other, or side by side, on tiers of driftwood.

The adult mummies showed openings, either in the lower part of the pelvis (perineum) or in the upper part of the thorax, through which presumably the internal organs were removed; but in the specimens found there were no remains of any stuffing.

Beneath or to the side of the mummies and burials there were repeatedly found, both in the caves and the rock shelters, cremated remains of humans, probably slaves, but mainly women and children.

The cultural materials recovered show a high degree of ability and even artistry, not excelled in similar lines anywhere else on the American or other continents.

The introduction of the practice of partial mummification of bodies in the Aleutian Islands must be attributed to the Aleuts; but where or when it originated remains a problem for future determination.

# THE WHITE DWARF STARS

By Dr. DIRK REUYL

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"Twinkle, twinkle, little star,
How I wonder what you are . . ."

SOME twenty-five years ago the first white dwarf star was discovered, a spherical mass of gas with a familiar white-hot surface, of some 8,000° absolute and a presumably acceptable interior temperature of several million degrees, but . . . of a density several thousand times greater than the earth's precious solid platinum.

In spite of refined researches with great telescopes, even to-day the total number known of this astounding type of stars is a mere score, a fact illustrating the difficulties met by the observational astronomer. Greater still are the obstacles confronting the theorist attempting to solve the riddle of the internal constitution of these stars. It must be said, therefore, that the problem is still far from a solution, and perhaps has not advanced beyond a rather elementary stage. However, from its spectacular beginning it has been a problem of paramount importance to physicists as well as to astronomers.

No doubt the problem presented by the white dwarf stars deserves even wider attention, in view of its close connection with theories of stellar evolution, theories of atomic structure, and other problems of more general interest. While writing this, I find that my contention is proved by a full page article in a recent issue of "Amazing Mystery Funnies." The reader will find there, immediately preceding the "Phantom of the Fair," the factual description of a white dwarf, interestingly adorned with pictures of steam shovels and derricks for moving ponderable things. Knowing that the white dwarf stars have penetrated the American home, I feel justified and encouraged to carry on with this article.

Let us go back, not a mere twenty years, but rather some twenty centuries and note that observations of stars' positions were made by Timocharis, Aristillus and Hipparchus, a few hundred years before the birth of Christ. When, in 1718, Halley compared these early positions, as given in Ptolemy's Almagest, with the observations made in his day, he discovered that small changes had taken place. The "fixed" stars are in very slow motion with respect to one another, and we have here the discovery of the "proper motions" of the stars, a phenomenon which Halley described as "not unworthy of consideration." Proper motion of a star is its apparent motion across the line of sight as seen from the earth.

The brightest star in the sky, Sirius, one of the stars studied by Halley, in a curious and devious way gave rise to the problem of white dwarfs under discussion. More than a century after Halley's discovery, Bessel, in 1844, announced the variability of the proper motion of Sirius, which he had suspected since 1834. Sirius while traveling through space, rather than going straight like a well-behaved star, was found instead to move along a wavy line; not in a short period or with large waves, but in 50 years and a barely observable variation from a straight line. The influence of a sinister companion would indeed explain the erring behavior of Sirius, for Sirius and a companion would revolve around their common center of gravity as they move through space. Only the companion, though quite systematic in its gravitative effects on Sirius, had been elusive from observers. Its position was neatly com-

puted by Safford, in 1861, and the next year it was actually seen close to its predicted position by Alvan G. Clark, the lens maker, in testing the refractor now at the Dearborn Observatory. Sirius and its companion, conveniently called Sirius A and B, then became easy prey for a score of investigators, all hungry for facts about this exceedingly interesting system. Soon the orbits of A and B about their common center of gravity were computed, as also their relative masses. The total mass, through Kepler's harmonic law, is known when we combine their known distance from us with the distance between them and the period of their revolution about their center of gravity. The determination of the parallax at the Leander McCormick Observatory is one of the several values obtained to anchor the star down in space. And also the apparent brightness was studied at McCormick by Dr. Vyssotsky, who was able to secure excellent photographs of this exceedingly difficult object, in close proximity to brilliant Sirius A but some 10,000 times fainter.

Let us now return to the results of the careful weighing of Sirius A and B. We find that the total mass of both stars is 3.39 times the sun's mass, Sirius A getting 2.44 and Sirius B 0.95, or almost that of the sun. And at once we notice how strikingly the near equality of the masses contrasts with the enormous difference in rate of radiation—a mass ratio of 3.7 to 1 and a radiation ratio of 10,000 to 1. Or, if we want to compare this faint companion with our sun, we have to explain the fact that Sirius B is more than 200 times fainter than the sun although it is of approximately the same mass. This difficulty could easily be overcome by assuming Sirius B to be a red dwarf star of so-called M type, *i.e.*, of low surface temperature, say about 3,000° absolute. Its inseparable friendship with the white star Sirius of type AO, corresponding to a surface temperature of 11,000°, need not worry us. Truly peace reigned supreme again after all the blame for Sirius' disorderly conduct had duly been placed upon its companion.

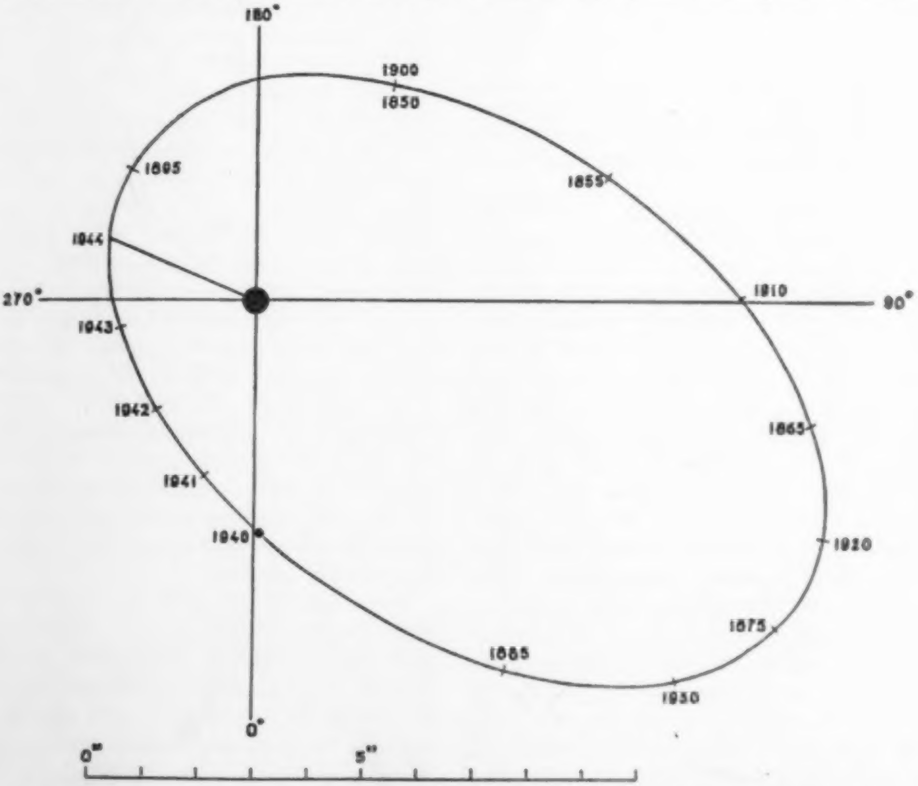
But this quiet proved to be only the lull before the storm which broke loose in 1915 when Adams made another startling discovery on this star. He succeeded in photographing the spectrum of Sirius B, a very difficult observation even with the great reflectors of the Mount Wilson Observatory. He classified the spectral type as A7, corresponding to an effective surface temperature of about 8,000°, and therefore of somewhat "later" type than Sirius A; in other words not quite so white hot, but with a touch of yellow. The world—that is to say the small brotherhood of astronomers on their planet, to be more specific—became badly upset by this announcement. Let us analyze why this sub-class No. 7 of type A caused such a commotion. We must first introduce the bolometric magnitude, a measure of the total radiation over the entire spectrum of a star, as contrasted with the visual magnitude, which gives the star's energy only in visual light, a rather limited range of wave-length in the yellow.

Sirius A, of type AO or effective surface temperature of 11,000°, has an intrinsic brightness given by the absolute bolometric magnitude 0.97 on a scale on which the sun has the value 4.85. Translating this into ordinary terms, we find that Sirius A radiates 36 times more energy than the sun. Since the effective surface temperature of the sun is 5,700°, we find that the radiation of Sirius A *per unit area* is 15 times that of the sun, since radiation per unit area is proportional to the fourth power of the temperature. Therefore the ratio of the areas of Sirius A and the sun is $36/15 = 2.4$, and, therefore, the ratio of their radii is the square root of 2.4, or 1.5. We are satisfied that

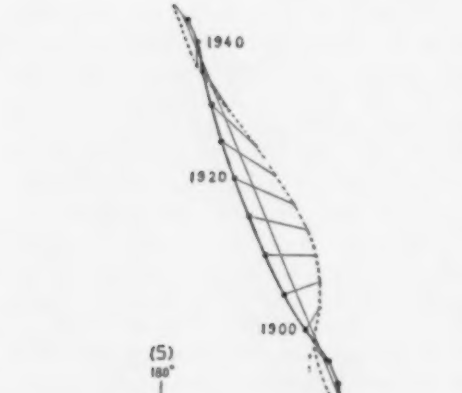
Sirius A with this radius and a mass of 2.44 solar masses may be considered a very reasonable star.

Let us now apply the same calculation to our trouble-maker, Sirius B. It is of type A7, or has a temperature of 8,000°. Comparing its faint absolute bolometric magnitude with that of the sun, we find that its energy output is only 1/360 that of the sun. Yet, because of its high temperature the radiation per unit area of Sirius B must be considerable; in fact, 3.8 times that of the sun. This leads to a ratio for the areas of $\frac{1/360}{3.8} = 1/1400$ and for the ratio of the radii 1/37. That is, Sirius B is found to be a dwarf star with a radius of only 19,000 kilometers, or less than three times the radius of the earth. This may sound startling, but matters become definitely alarming when we realize that an amount of gaseous matter practically equal to the mass of the sun is crowded into a sphere with a volume 50,000 times smaller than that of the sun. If we have managed up to now to stand up under the strain, we may sit down and calculate the densities of Sirius A and B. We find that the average density of Sirius A is about that of water, but that the little Sirius B has a density about 70,000 times as great—a cubic inch of it at the surface of the earth would weigh roughly a ton.

At this point Eddington admitted that it would seem reasonable to dismiss the conclusion as absurd. However, he chose to attack the problem theoretically. Among the first approaches was to find whether the certain consequences of Ein-




APPARENT ORBIT OF SIRIUS B AROUND SIRIUS A. THE PERIOD OF REVOLUTION IS 50 YEARS.




WAVE MOTIONS OF SIRIUS A (FULL CURVE) AND SIRIUS B (DASHED CURVE). THE CENTER OF GRAVITY OF THE SYSTEM TRAVELS ALONG THE STRAIGHT LINE AT UNIFORM SPEED.

stein's theory of relativity actually existed. When light waves pass through a gravitational field their frequency is decreased; in other words lines in the spectrum will be shifted toward the red. Since the effect is proportional to the mass and inversely proportional to the radius of the attracting body, we find that Sirius B provides an excellent test case, because the predicted effect is 34 times that of the sun. This would produce a shift in the spectral lines corresponding to a velocity in the line of sight of about 20 kilometers per second. Fortunately we are dealing with a double star system, and hence by making differential measures between the spectral lines of A and B, the line of sight velocity of the system as a whole does not enter into the picture. Adams observed a shift of 23 kilometers per second, which leaves us, after allowing for 4 kilometers per second due to orbital motion, with 19 kilometers per second as the observed relativity displacement. As Eddington explained, the observation by Adams killed two birds with one stone, not only in giving a splendid confirmation of Einstein's relativity displacement, but also in proving the extremely dense condition of Sirius B.

How to explain this condition is another matter. Eddington reasoned that the only satisfactory solution would be in assuming the gas to be in a state of complete ionization; that is, the atoms are not only stripped of their outer electrons but of their inner ones as well. Under normal conditions the electrons remain in their places and keep their proper distances, leaving a great deal of empty space in the atom of which they are members. Under extreme ionization they become independent individuals and are free to crowd together into the superdense material which constitutes the white dwarf interior. At once Eddington was confronted with a new riddle. How can the gaseous matter cool down and finally turn into the state of an ordinary solid composed of atoms? In the change into atoms the star will have to expand to a radius ten times larger against the force of gravitation, and therefore it will *require energy in order to cool!* From where is this energy expected to come when ultimately the supply from the interior becomes exhausted?

Quoting Eddington, we must indeed "imagine a body continually losing heat but with insufficient energy to grow cold!" "I do not see," remarked Eddington, "how a star which has once got into this compressed condition is ever going to get out of it."

The solution as given by Fowler in 1926, sounds simple enough. The star never does get out of this condition, but with its high density and relatively low surface temperature must be considered a gas in "degenerate" state. Work by Fermi, Dirac, and later by Lindemann, has been utilized by Milne, who points out that the ultimate fate of the particles

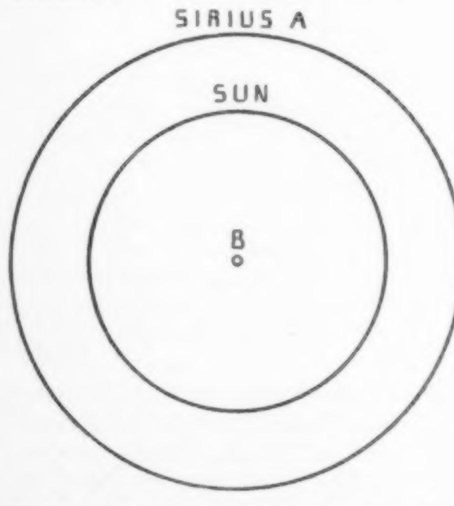
in such a state is complete organization. We shall then have a manifestation of the highest degree of atomic order and regimentation. "Freedom is non-existent, the final state represents atomic civilization in its highest form—or its lowest form, if my hearers prefer," quoting Milne from his Halley lecture, delivered on 19 May, 1932. According to Fowler, the white dwarf at the end of its life, in other words, completely degenerate and at the absolute zero, is analogous to one gigantic molecule in its lowest quantum state. The meaning of temperature has vanished; it may well be called zero.

The reader will observe that this final state offers a condition for which the extent of ionization may at the same time be called complete and zero! The particles, first liberated under a communistic white dwarf régime can only find that ultimately they must die in complete subordination, to the totalitarian state of the "black" dwarf. To reach this final stage of "black" dwarf, Milne suggests that the star in its white dwarf state consists of a degenerate core surrounded by a shell of ordinary gas. This non-degenerate shell, in cooling would exhibit the gradual reddening of the star. Simultaneously, its composing matter would gradually turn from the normal into the degenerate state, until finally the entire star had become degenerate.

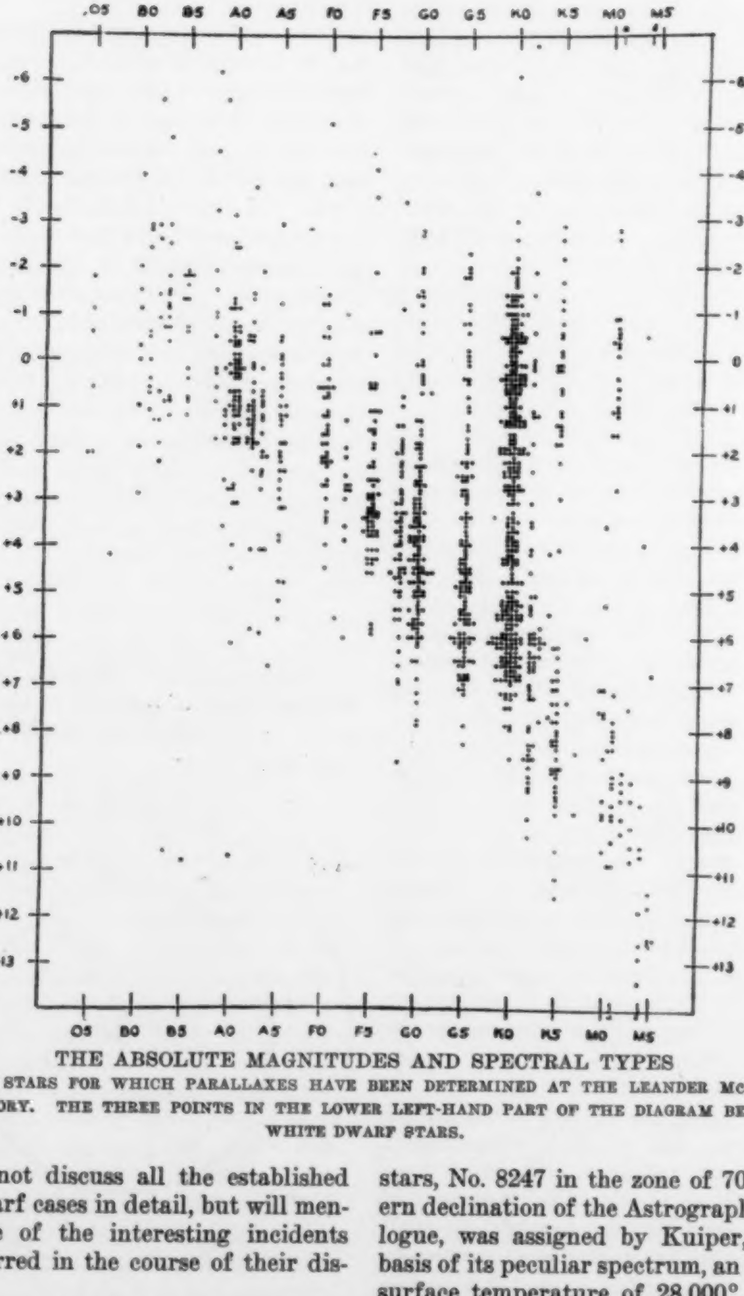
We must now do great injustice to further valuable theoretical investigations by leaving these aside in order to use the remaining space for a review of important recent observational work.

In surveying the spectra of the known nearer stars, and of those suspected of being near-by, Kuiper and others have discovered a considerable number of white dwarf stars. Because of their small size, the stars are faint, in spite of their comparative nearness, as stellar distances go, making the difficulty of detection very great. Whiteness of these faint, presumably near-by stars is, therefore, the first criterion. Spectral characteristics, such as lines widened by the Stark effect and of unusually great intensity in the violet, are practically conclusive. If the star lies within reach of the great "parallax" telescopes, the determination of its distance will serve to settle matters definitely. Not only for Sirius B, but also for four other white dwarf stars, was the parallax determined at the McCormick Observatory. For two of Kuiper's recently discovered stars the distances, substantiating the white dwarf character, have been determined by the writer. Several more stars are under observation, leaving untouched only those for which, on account of their extreme faintness, exposure times would become prohibitive.

The white dwarfs found up to date, though small in number, show conclusively some degree of variety in their spectra, some being bluer, others more yellow. Knowledge of their intrinsic luminosities, from apparent brightness and parallax, is imperative in order to study their place in the scheme of stellar evolution.




RELATIVE DIAMETERS OF SIRIUS A, THE SUN AND SIRIUS B. SIRIUS A HAS A MASS 2.4 TIMES THAT OF SIRIUS B WHICH IN TURN HAS A MASS EQUAL TO THAT OF THE SUN.




THE ABSOLUTE MAGNITUDES AND SPECTRAL TYPES

FOR 1206 STARS FOR WHICH PARALLAXES HAVE BEEN DETERMINED AT THE LEANDER MCCORMICK OBSERVATORY. THE THREE POINTS IN THE LOWER LEFT-HAND PART OF THE DIAGRAM BELONG TO WHITE DWARF STARS.

I will not discuss all the established white dwarf cases in detail, but will mention some of the interesting incidents that occurred in the course of their discovery.

There are, for instance, some cases where the spectrum is entirely devoid of lines, just continuous. One of these stars, No. 8247 in the zone of 70° northern declination of the Astrographic Catalogue, was assigned by Kuiper, on the basis of its peculiar spectrum, an effective surface temperature of 28,000°. Since the distance of this star had been determined, its intrinsic brightness could be derived. Realizing that the latter is pro-

portional to the square of the radius and the fourth power of the surface temperature, this relation was utilized to derive the value of the radius. It was found to be equal to *one half of the earth's radius*, making this star the smallest known. A direct determination of the mass was not possible, but from the theoretical relation between radius and mass derived by Chandrasekhar for degenerate gas spheres, the mass could be calculated and was found to amount to 2.8 solar masses. This amount of matter, then, is all packed in a sphere with only half the earth's radius. It can hardly be a surprise any more that the figure for the density assumes staggering dimensions. We find it to be 36 million times the density of water, weighing a mere 620 tons per cubic inch if at the surface of the earth. Incidentally, the force of gravity on the surface of the star is 3½ million times that on earth.

Of all white dwarfs, only three are near-by stars, situated within 16 light years of the sun. In fact, all the others are beyond 40 light years. If we assume that no more stars will be found within this limit, we can calculate the relative frequency of the white dwarf stars. The frequency of all stars in the neighborhood of the sun, that is, within 25 light years, is fairly well known from various methods of attack. If we adopt one of the more recent determinations, that by van Maanen, the white dwarfs are found to constitute about one per cent. of all stars. However, it seems likely that this estimate may be much too low; in fact, the white dwarfs may prove to be quite abundant. At any rate, whether of exceptional type or not, they will continue to occupy a first rank position among astrophysical problems.

Much research remains to be done, not only with regard to the white dwarfs as such, but also to their relation to other stars, notably the relatively dense nuclei of planetary nebulae and the "end-products" of the exploding novae. The spectral features in common between these stars and the white dwarfs would point to a possible relationship. Their densities, however, though of the highest among the "normal" stars, are no match for those of the extremely compressed white dwarfs.

Possibly further insight into the properties of super-dense matter may lead to a better understanding of the constitution of the more normal stars. If the problem of stellar evolution is to be solved, we must fit into its scheme not only these super-dense stars, but also the massive super-giants in highly diffuse states, the unstable configurations presented by the pulsating Cepheid variables, etc.

Perhaps the "gap" in luminosity between the normal intrinsically bright white stars and the faint white dwarfs may eventually be filled, depending on the outcome of extended spectral analyses and distance determinations. For this purpose we at the McCormick Observatory are determining the distances of all white stars which, on the basis of proper motion and apparent brightness, show a promise of being "intermediates."

The figure taken from Volume VIII of the Publications of the Leander McCormick Observatory will serve to illustrate this further. In this familiar Russell-Hertzsprung diagram, 1206 stars are plotted according to their spectral types and absolute magnitudes—as a measure of their luminosities—based on determinations of their parallaxes with the McCormick refractor.

At the first glance the diagram may appear as the cold, matter-of-fact statistics of 25 years of continuous parallax research. However, looking more intently, we shall read in it a vivid description of the variety of brilliance and color of a sidereal Broadway. At once conspicuous features attract our attention, the near equality in brightness of the most brilliant displays, the stars on the

"giant" branch, regardless of their color; the gradual dimming of the less assuming lights, the stars on the "Main Sequence," as we pass from the bluest, along orange and yellow to the reddest members; and last but not least the feeble, unjustly modest display of the white dwarfs, in splendid isolation.

It must be stated that Sirius B, among others, does not appear on the McCormick diagram, as its distance was not determined directly, though it is known accurately through measures of Sirius A. However, the inclusion of the omitted white dwarfs would not materially alter but would rather strengthen the position of this small group of B and A type stars of low luminosity.

From the evolutionary point of view it can be stated with certainty that the diagram as a whole represents a set of loci of equilibrium points, each point representing some particular stage in the process of stellar evolution. As to the course of travel there may well be a multitude of tracks on this emplacement. Presumably abundant will be the course of gradual changes of luminosity, color, mass, etc., along the same or parallel tracks. Then again we may have to deal with unruly bodies which would at times prefer to jump their tracks in order to proceed on others.

Valuable contributions to the interpretation of the Russell-Hertzsprung diagram in recent years have been made by Strömgren and others, through studies of the hydrogen content masses and radii of the stars.

As to the white dwarfs, the importance of observational work such as Kuiper's need hardly be emphasized, and no less that of those who, through equally painstaking research, provided the suspects, the relatively small group of stars of large proper motion. Discoveries of new white dwarfs, extended studies of their spectra and determination of their intrinsic brightnesses will serve not only to add to our knowledge of these objects, but also may throw new light on their relations to the other stars.

Then, also, there may come an announcement of the discovery of the first yellow or perhaps red "subdwarf," possibly cases of former white dwarfs well on their way to their ultimate fate, the "black" dwarf state of death of all super-dense matter.

These types of stars, however, are still brain-children of theorists, and to some degree products of wishful thinking and dabbling in speculation. Whether or not they are missing links in the evolutionary scheme and do exist in space, time only can tell.

# HEREDITY AND THE LAWYER

By Dr. ALEXANDER S. WIENER

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In recent years, judges, juries and lawyers have become more receptive to the application of scientific knowledge for the solution of problems arising in courts of law. Still fresh in the minds of most American citizens is the Lindbergh kidnapping case, in which an expert on wood linked Bruno Hauptman with the crime by showing that the wood in the ladder used in the kidnapping had been taken from the floor of the attic in the Hauptman home. The Ruxton case in England, in which portions of the dismembered bodies of Mrs. Ruxton and her nurse-maid were successfully pieced together and identified, is another good example of the successful application of scientific methods in a criminal case.

Not infrequently courts of law are confronted with cases where a knowledge of genetics is of value. For instance, the decision of the court may depend on whether or not a particular individual is the child of a certain man and a certain woman. The most common examples are paternity proceedings, where the child has been born out of wedlock and the mother claims a particular man to be the father. In these cases, it is important to fix properly the responsibility for the support of the child, who might otherwise become a public charge. In divorce cases, the husband may assert his wife to be guilty of adultery, and as evidence of such adultery endeavor to prove that he can not possibly be the father of the child born during their marriage. In inheritance cases, impostors have put in claims by posing as long-lost heirs to the estate. The most notorious example in recent years of such an unsuccessful attempt is the Wendel case.

In the thirteenth century problems of blood relationship were claimed to be "solved" in Japan and China by the "blood-dropping test," in which drops of blood of the individuals being tested were allowed to fall into water simultaneously; if the drops came together the conclusion was that a relationship existed. As recently as 1929, a counterpart of this naïve test was invented by Zangemeister. In Zangemeister's test the sera of the child and the putative father were mixed and the mixture examined in a photometer for an increase in turbidity, the occurrence of which was supposed to be proof of paternity. A similar turbidity was supposed to appear in mixtures of the sera of husband and wife, but mixtures of sera of unrelated individuals were said to remain perfectly clear. Zangemeister stated that this supposed phenomenon was the result of the immunization of the mother and fetus in utero to the sperm of the father. This test and a similar one invented by Zangemeister for the diagnosis of pregnancy were "successful" only in his own hands, so they have been relegated to the same category as the tests used in Japan and China in olden times.

A more valid method of establishing familial relationship and at the same time among the most ancient is by demonstrating a facial resemblance of the child to its parents. This phenomenon is so common that it hardly requires discussion. The most outstanding example is the resemblance between a pair of *monovular* or so-called "identical" twins. Such twins result when a single ovum fertilized by a single sperm, instead of developing into a single individ-

ual, splits in half, and each half develops into a separate individual. On the other hand, *biovular*, or "fraternal" twins develop from entirely different fertilized eggs and therefore, aside from the fact that they are of equal age, are no more alike than ordinary brothers and sisters. Monovular twins are as much alike and no more different than the two sides of the body, so that it is difficult for strangers to tell one from the other. So great was the faith of the ancient Carthaginians in resemblance as a criterion for determining parentage that all children at the age of two months were examined by a special committee, and if the resemblance to the father was not great enough they were done away with.

In courts of law the resemblance between the child and its supposed parents has frequently been advanced as evidence of parentage. In the Wendel case the claimant to the estate pointed out the similarity between his own features and those of a bust of the deceased. Establishing parentage by resemblance has, however, many serious limitations. When the likeness is particularly striking, as in the case of identical twins, little doubt would seem possible, but as a rule the resemblance is not so strong. In ascertaining resemblances there is a strong subjective element, particularly with infants and young children whose features are not fully formed. Moreover, features will change as a result of age, diet, disease, injuries, etc., and in this way one can easily be misled. Two closely related individuals may appear entirely different; two homely parents can have beautiful children, and two beautiful parents can have homely children.[1]

On the other hand, two totally unrelated individuals may strikingly resemble each other, as in Mark Twain's novel, "The Prince and the Pauper," and there have been such instances reported in the daily press, involving prominent personalities. Resemblance is even more difficult to ascertain when comparison is made not between two living individuals but between two pictures or busts. For these reasons, many courts of law, such as those in New York State, do not permit the exhibition of the child to the jury for purposes of comparison with the putative father in cases of disputed parentage. Indeed, such a proceeding would merely serve to arouse the emotions of the jury and prejudice them against the defendant, rather than permit a sober unbiased appraisal of the situation.

The main cause for the difficulties entailed in applying resemblance as a mode of establishing parentage is that the features are the complex result of the many separate characteristics which enter into it, each of which has its own independent inheritance. A more scientific approach to the problem, and a more objective one, is through the use of so-called "unit characters."

As Mendel first pointed out at the middle of the nineteenth century, such unit characters are transmitted by determiners now known as genes. As an example of a unit character let us consider the color of the eyes. For simplicity, eye colors can be classified as dark and light. These characters are inherited by means of a pair of allelic genes, which may be designated by the letters *d* and *l*, respectively, where *d* represents the gene for dark eyes, *l* the gene for light eyes. Since each individual has in his somatic cells two genes for each unit character, one from the father and the other from the mother, there are three genotypes possible with respect to the genes *d* and *l*, namely, *dd*, *ll* and *dl*. Obviously, individuals of genotype *dd* will have dark eyes and individuals of

[1] *Cf.* A. Scheinfeld, "You and Heredity," Chapter XXVIII. New York: F. A. Stokes Company. 1939.

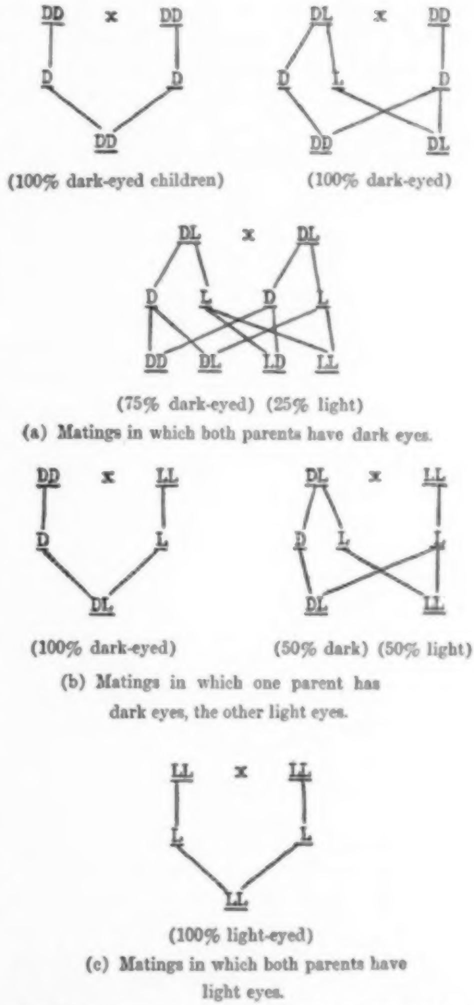
genotype *ll* will have light eyes. With regard to the individuals of genotype *dl*, Davenport has shown that the gene for dark eyes is dominant over the gene for light eyes, so that such individuals will have dark eyes.

With the aid of Davenport's theory of the inheritance of eye color it is possible to predict the colors of the children's eyes if those of the parents are known. There are three matings possible: (1) both parents dark-eyed, (2) one parent dark-eyed and the other light-eyed and (3) both parents light-eyed. Let us consider, for example, the matings where both parents have light eyes. In these cases both parents are of genotype *ll*, and as every germ cell contains one and only one gene from each allelic pair, all the germ cells of both parents will contain gene *l*. At fertilization, therefore, only zygotes[2] of genotype *ll* will be produced and all the children will have light eyes. The other matings are worked out in a similar way, but one must bear in mind that when a parent has dark eyes his or her genotype may be either *dd* or *dl*.

The results of Davenport's theory may be summarized as follows: (1) when one or both parents have dark eyes the children can have either dark or light eyes; (2) if both parents have light eyes none of the children can have dark eyes. The rule of practical importance is the second, since on this basis it would theoretically be possible to prove that a given individual is not the father of a certain child. If a woman has light eyes and her child dark eyes, then no man with light eyes could be its father; on the other hand, if the accused man in such a case had dark eyes, that would not necessarily prove that he was the father of the child, since a large percentage of individuals have dark eyes.

Unfortunately, there are a number of serious obstacles to the reliable applica-

[2] Fertilized ova.




(100% dark-eyed children) (100% dark-eyed)

(75% dark-eyed) (25% light)

(a) Matings in which both parents have dark eyes.

(100% dark-eyed) (50% dark) (50% light)

(b) Matings in which one parent has dark eyes, the other light eyes.

(100% light-eyed)

(c) Matings in which both parents have light eyes.

INHERITANCE OF EYE COLOR IN MAN

GENOTYPES OF PARENTS ARE INDICATED BY LETTERS AT TOPS OF DIAGRAMS; THE GAMETES (GERM CELLS) BY LETTERS AT CENTERS AND THE GENOTYPES OF THE CHILDREN AT BOTTOMS.

tion of Davenport's theory of heredity of eye color in medico-legal cases. First of all, the simple classification of eye color as dark and light does not correspond with the real state of affairs. Actually, a rather large variety of colors and shades exist, such as brown, black, gray, hazel, blue and green; in albinos

the eyes may be pink. To allow for these possibilities it is necessary to postulate the existence not of a pair but of multiple allelic genes. Moreover, the dominance of the dark colors, brown and black, over the lighter colors, gray, blue and green, is not absolute, so that occasionally individuals of genotype *dl* may have light eyes instead of dark eyes. If two such light-eyed individuals intermarry they might have a dark-eyed child, thus upsetting the rules of hereditary transmission. Finally, the eye color does not remain constant throughout life. In newborn infants the eyes are usually blue or some other light color, but later on they may change to a darker color such as brown; moreover, one's eye color may change as a result of disease involving the iris, or in old age.

When we turn to other normal physical characters we encounter similar obstacles to their application in problems of parentage. For example, though there is no doubt that the type of ear-lobe, whether large and free or small and attached to the side of the head, is hereditary, the mechanism of transmission is not clear-cut, so that it is not possible to predict with absolute certainty what type or types of ear-lobes the children will have when those of the parents are known. The same may be said for the finger-prints, the use of which in paternity disputes has been advocated by certain investigators (Nurnberger, Bonnevie). It might seem that the finger-prints would furnish the ideal solution in problems of disputed parentage in view of their pronounced individuality. However, it appears that the mechanism of heredity of the finger-prints must be almost as complicated as the finger-prints themselves, and even to-day their heredity is not clearly understood. One limitation is that the finger-prints on the right and left hands of the same individual may be quite different (as are the finger-prints of identical twins), although such prints show a closer resemblance than the prints from unrelated individuals.

In fact, hardly any of the normal individual differences among human beings visible to the unaided eye have as perfectly simple an inheritance as that described by Mendel in his classic studies on the sweet pea. There are, however, individual differences among normal humans which are not visible to the naked eye, but are of a biochemical nature which exhibit a simple Mendelian inheritance. The most important of these characters are the blood groups, O, A, B and AB. The existence of individual differences in human blood was discovered by Landsteiner in 1900–01, who with Levine also discovered the so-called human blood types, M, N and MN, in 1928.

What blood group a person belongs to is determined by testing his red blood cells for two substances known as agglutinogens A and B, respectively. The chemical nature of these substances is not completely understood, though they seem to be related to polysaccharides, and no physiological function has been found that they perform. Their presence or absence in the blood is determined with the aid of two sera, one containing agglutinin anti-A, the other anti-B, which act on blood containing agglutinogens A and B, respectively. If the agglutinogen in question is present in the cells, the cells will clump together (or agglutinate) into large masses; if the agglutinogen is absent, no clumping of the cells occurs. If no agglutination occurs in either serum, the group is O; if clumping occurs only with the anti-A serum, the group is A; if clumping occurs only with the anti-B serum, the group is B; and if clumping occurs with both sera, the group is AB.[3]

[3] These reactions are the basis of the fatal sequelae which may result if blood of the improper group is administered in a blood transfusion.

As Bernstein has shown, the inheritance of the blood groups is determined by a series of allelic genes A, B and O, where genes A and B determine agglutinogens A and B, respectively, and are dominant over gene O.[4] Corresponding to the four blood groups, therefore, six genotypes are possible, as follows: Group O—genotype OO; group A—genotypes AA and AO; group B—genotypes BB and BO; group AB—genotype AB. It is a simple matter to ascertain what groups can occur in the children when the groups of the parents are known, in the manner outlined when discussing the inheritance of eye-color. Ten matings are possible, and these together with the children possible are given in Table 1.

TABLE 1

THE LANDSTEINER BLOOD GROUPS IN PARENTS AND CHILDREN

| Groups of parents | Groups of children possible | Groups of children not possible |
|---|---|---|
| 1. O × O | O | A, B, AB |
| 2. O × A | O, A | B, AB |
| 3. O × B | O, B | A, AB |
| 4. A × A | O, A | B, AB |
| 5. A × B | O, A, B, AB | |
| 6. B × B | O, B | A, AB |
| 7. O × AB | A, B | O, AB |
| 8. A × AB | A, B, AB | O |
| 9. B × AB | A, B, AB | O |
| 10. AB × AB | A, B, AB | O |

For those interested primarily in the application of blood grouping in cases of disputed parentage, it is sufficient merely to remember the following two laws of inheritance: (1) Agglutinogen A or B can not appear in the blood of a child unless present in the blood of one or both parents. (2) A group AB parent can not have a group O child, and a group O parent can not have a group AB child.

To illustrate how this knowledge is applied, a case will be described in which a mixture of babies occurred in a Chicago hospital in 1930, the problem finally being solved by the blood grouping tests. Mr. and Mrs. B., on returning home from the hospital with their baby, noticed that it bore a label on its back with the name "W." They immediately hurried to the home of Mr. and Mrs. W. and it was found that the baby there had a label "B." on its back. The poor parents were in a quandary, not knowing whether they had taken their own babies home or the labels on the infants' backs were correct, and they sued the hospital for damages. The court ordered blood tests to be made and the findings were as follows:

| *Blood of:* | *Group:* | *Blood of:* | *Group:* |
|---|---|---|---|
| Mr. B. | AB | Mr. W. | O |
| Mrs. B. | O | Mrs. W. | O |
| Baby "W." | O | Baby "B." | A |

Since two parents of groups AB and O, respectively, can have only children of groups A and B, but not of group O or AB, it is evident that the baby with the label "W." on its back could not possibly be the child of Mr. and Mrs. B.; on the other hand, the baby labelled "B." could be their child. Moreover, since two group O parents can only have group O children, Mr. and Mrs. W. could not possibly be the parents of the baby labelled "B." but could be the parents of the other child. In this way the blood grouping tests solved the vexing problem and, by order of the court, the children were exchanged and restored to their own parents.

It is evident that with the aid of the blood tests it is possible to assert only that a given individual *can not be* the father of a given child in those instances where the groups do not conform with the laws of inheritance cited above. It is not possible to assert with certainty that a certain person *is* the parent of a given child, except where it is known that one out of a few individuals only could be the father, and all but one of these are excluded by the tests. When a man has been unjustly accused of the

[4] The heredity of the blood groups has been compared to that of the color of flowers and to that of the color of the eyes.

paternity of a given child, his innocence can be established with the aid of the blood groups in about one sixth of the cases. The number of such cases which can be solved was doubled by the discovery in 1928 by Landsteiner and Levine of two additional agglutinogens of human blood, designated by the letters M and N. These properties, which are entirely independent of the agglutinogens A and B, determine three types of blood, M, N and MN, and are transmitted with the aid of a pair of allelic genes, M and N. Corresponding to the types the following genotypes exist: type M—genotype MM; type N—genotype NN; type MN—genotype MN. On the basis of this theory, the inheritance of agglutinogens M and N is as given in Table 2. For use in the courtroom it is

TABLE 2
THE AGGLUTINOGENS M AND N IN PARENTS AND CHILDREN

| Types of parents | Types of children possible | Types of children not possible |
|---|---|---|
| 1. MN × MN | M, N, and MN | ——— |
| 2. MN × N | N and MN | M |
| 3. MN × M | M and MN | N |
| 4. M × N | MN | M and N |
| 5. N × N | N | M and MN |
| 6. M × M | M | N and MN |

sufficient to remember the following two laws: (1) The agglutinogens M and N can not appear in the blood of a child unless present in the blood of one or both parents. (2) A type M parent can not have a type N child and a type N parent can not have a type M child.

One case will be cited to illustrate the successful application of the agglutinogens M and N for solving a problem of disputed parentage. A woman sued her husband on account of non-support. He counter-claimed with a suit for annulment on the ground that he was not the father of his wife's child, but had been led to marry her by her false assertion that the child was the result of one of their clandestine meetings during her previous marriage. When the bloods were examined by the writer, no definite conclusions could be drawn from the tests for A and B. However, it was found that the woman in question belonged to type N and the child to type M, so that she could not be the mother of her supposed child. Further investigations revealed that the woman in the case had been married six times previously, and some old hospital records were found which revealed that she had had an operation some time previously which made it impossible for her to have had a child. Her contention was that the midline scar on her abdomen was the result of a Caesarean operation, though she could not produce the surgeon who was supposed to have performed the operation. Finally, the orphanage was located from which she had adopted the child which she used to perpetrate the fraud on her present husband.

Other cases in which blood grouping tests can be applied are inheritance disputes, divorce and rape cases, and kidnapping cases. Where litigation is anticipated in inheritance cases it may even be wise to take blood grouping tests on the deceased at the time of death. Of special interest are problems of paternity involving twins. If the twins are monovular they must have come from the same father, of course. In the case of fraternal twins, however, it is theoretically possible for each, while it has the same mother, to have a different father. This occurrence is known as superfecundation, and is scientifically possible though most difficult of proof. In 1934 Judge A. B. Tripp, sitting in Yankton, granted a divorce to a man on the grounds of infidelity. The man requested and obtained custody of the twin who looked like him, and the wife was left with the twin who looked like the neighbor. A more scientific conclusion would have been possible in this case had blood tests been made and it had been

shown that the claimant was not the father of one of the two twins. According to *Time* magazine,[5] American medical records of the last century contain reports of cases of two white girls, each of whom cohabited in rapid succession with a Negro and a white man. The result was that each bore twins, one of which was white, the other mulatto.

As mentioned above, a falsely accused man can be exonerated by means of the four blood groups in one sixth of the cases, and his chances have been increased to about 33 per cent. by the discovery of the properties M and N. The question may arise whether by the discovery of additional blood factors the percentage of successful cases can be raised to 100 per cent. Theoretically this ideal can be approached but not reached, since with each new factor that is tried there is overlapping with the older blood tests, some men being excluded by more than one of the blood tests. It might be mentioned that additional properties in human blood besides A, B, M and N have been discovered which can be used in problems of disputed parentage, but they have not been studied enough to warrant their use in the courtroom at the present time. Where one is asked to offer a private opinion as to paternity their use might be permissible, if the required reagents are available. The more important of these factors are the agglutinogens $A_1$ and $A_2$ (varieties of A agglutinogen), the agglutinogen Rh and agglutinogen P. In addition, people of groups A, B and AB may secrete group specific substances in their saliva, and the capacity to secrete them is hereditary. While morphological traits such as eye color, dimples in the chin, hair color, etc., do not exhibit as clear-cut an inheritance, in private consultations they may be used for the purpose of arriving at an opinion, though not an absolute decision, as to the paternity of a child, in cases where the result of the blood tests are inconclusive.

[5] *Time*, Jan. 8, 1934.

Aside from the normal individual differences there are numerous abnormal anatomical and physiological anomalies which are hereditary, such as polydactylism, claw hand, hemophilia, albinism, etc. These abnormal traits are mostly inherited as simple unit characters in accordance with the Mendelian laws, and therefore can be used in paternity proceedings should they occur in parents and children. In fact, because of the rarity of such anomalies, their simultaneous presence in the putative father and child may be taken as strong circumstantial evidence that the man in question actually is the father of the child. Mohr cites a case in which the presence of brachyphalangy (short fingers) was the basis of a court's decision that the man in question was actually the father.

Until recently one of the major impediments to the more general acceptance of the blood grouping tests in the courts of this country has been the lack of suitable legislation giving the courts power to compel individuals in such proceedings to submit to blood examinations. Suitable laws have, however, been passed in New York, Wisconsin, New Jersey, Ohio and Maine, and similar legislation is pending in other states. Another difficulty is the lack of a sufficient number of qualified individuals in each state to carry out such examinations. The conducting of a blood grouping examination requires rather highly specialized knowledge, and erroneous reports have been rendered where inexperienced individuals were permitted to carry out the tests.[6] But bloods can be drawn in one locality and shipped through the mails to individuals qualified to carry out the examination. In one such case the

[6] *Cf.* Report of the Committee on Medico-legal Blood Grouping Tests, *Jour. Amer. Med. Assoc.*, 108: 2138, 2115, 1937.

writer in New York received bloods of a mother and child 24 hours after the birth of a baby in a hospital in Colorado, the man's blood being taken in New York. Since the blood had been shipped by air mail and packed in ice, the cells were practically fresh when received, despite the great distance involved, and the examinations were conducted with no greater difficulty than if the individuals had all put in a personal appearance.

In conclusion the writer would like to point to a recent decision of the United States Court of Appeals for the District of Columbia. In this case, involving the paternity of a baby born in wedlock, the court issued an order directing a man, wife and child to submit to blood grouping tests, and this order was affirmed by the Court of Appeals, which accepted as established the scientific value of the blood tests. An editorial which appeared in a recent issue[7] of the *Journal* of the American Medical Association, commenting favorably on this decision, closed with the following sentence: "Granting the right of the court to compel submission to blood grouping tests, either by authority of a special law . . . or under a more embracive statute authorizing the court to compel submission to physical examination without specifically mentioning blood grouping tests, as was the situation in the recent District of Columbia case, there would seem to be no justification for further hesitancy on the part of the courts to accept as scientifically sound the results of blood grouping tests to the extent that they disprove the possibility of paternity."

[7] *Jour. Amer. Med. Assoc.*, 115: 306, July 27, 1940.

# A PHYSICIST'S VIEW OF ETHICS

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Professor Conklin, in his article "Does Science Afford a Basis for Ethics?" in the October, 1939, issue of The Scientific Monthly, showed that science does afford a basis for ethics but did not go ahead to outline, as I shall try to do, a theory which can serve as the skeleton of a science of ethics. Conklin is right in saying that ethics is natural in origin and has undergone an evolution from simple to complex. Further, such a natural development of ethics is more hopeful than any supernatural development could be. To a limited extent I agree that "As one goes up through higher and higher social grades one finds that altruism reaches farther and takes in more people, until with some persons it includes the whole human race"; but I think that Conklin is using the word altruism a bit loosely here. It seems better to say that as the minds of persons become more highly developed their interest in, and sympathy for, others broadens, but this interest and sympathy is never really altruistic.

Ethics becomes more understandable to me after making an analysis of its supposed base: altruism. When one studies the evolution of ethics in particular, and human behavior in general, it seems clear that selfishness is universal, obvious in many cases, disguised in others, unavoidable in all. Selfishness is a good thing simply because it is necessary for the preservation and growth of living things. Many will say that I am "unethical" in acknowledging the pursuit of pleasure or happiness as my sole aim in life, but they misuse the

word. They would be speaking more accurately if they said that a hedonistic code of ethics conflicts with their codes of ethics.

Pleasure is one of those things which are too fundamental to define in terms of simpler things; one can only define it by exemplification. In general, one may say that the exercise of any normal function of the human body produces pleasure. That statement is perhaps as much a definition of normal function as of pleasure. Eating, drinking, resting and sexual intercourse are examples of obviously normal bodily functions that are normally pleasurable. If a person does not enjoy such elementary actions in reasonable amounts, something is wrong with him or her, physiologically or psychologically. What other experiences one considers pleasant is dependent on one's mental development, training and habits, and in some cases these experiences may be fantastic and apparently unexplainable. Thought or consciousness may be considered to be the highest human function or bodily process; its performance, accordingly, should give the highest pleasure. The fact that many people say that "thinking is the hardest work to do" indicates how poorly developed this function is in them. One who does not enjoy thinking has not done much of it, for thinking is like a game which one enjoys more, the more practice one has had and the better one can play. Thinking is, moreover, a pleasure which, like eating, may be indulged in unwisely. Overthinking on a useless subject, or daydreaming, can waste time just as overeating can cause indigestion.

One may say that the highest type of thinking is that type called creative, although all thinking is to some extent creative. Even a student carefully following the train of thought of the author of a text-book is creating anew for himself the ideas which may have been possessed by millions before but are yet fresh and unexplored to him. There are three requirements for creative thinking: First, the mind must have ideas to work with; these require experience and reflection to form. Second, the mind must have imagination, the ability to put these ideas together in new combinations. Third, the mind must have the ability to recognize the value in certain of these combinations which may appear to have been formed as if by accident without conscious desire or direction. All three of these phases of creative thinking have evolved naturally from lower forms of nervous and mental activity. It may then be expected that the exercise of imagination will bring pleasure. This is found to be true; indeed, for some people the highest type of pleasure is obtained from imaginative thinking. For instance, the use of imagination in science, primarily in the construction of theories, is an essentially selfish act performed to gain pleasure, as is every other mental act. This is often indirectly admitted by the scientists themselves when they speak of the "beauty" and "elegance" of their theories. To one able to appreciate it, the beauty of a far-reaching theory may be as great as that of the best painting or sculpture. The pleasure derived from the creation of the theory may also be as great as that enjoyed by the painter or sculptor or musician, and is of the same intellectual or mental type.

Even the mass of unscientific people, who have never consciously created a scientific theory, and who think that "facts" are superior to "theory," do a great deal of theoretical thinking, and get pleasure from it, without realizing the nature of what they are doing. The things that are called facts by those persons who claim to be interested only in "practical facts" always involve some theory of the world which is so commonly accepted that its theoretical na-

ture is unnoticed. The common idea of an antithesis between theory and facts, or between theory and practice, is an unfortunate misconception. The most practical and labor-saving thing in the world is a good theory, one that has been verified and found to work. As for facts or knowledge, we have only a set of sense impressions which is organized to a limited extent by means of ideas and theories of varying degrees of simplicity and clearness. The thing that, alone, makes sense impressions have meaning is that purest and most valuable of all fictions, the physical world, which we postulate as the cause of the perceptions. Without this fiction our perceptions are meaningless; in fact, we would not even have what most people, including most philosophers and psychologists, call crude sense impressions, but only have vague feelings of comfort or discomfort. For example, one can not recognize that the sensation produced by breathing ammonia vapor is a smell, not merely an unpleasant feeling of unknown origin, without having had previous experience with ammonia and the sense of smell, not even without something of a theory of the structure of the universe and of mankind's place in it.

Imagination and creativeness are usually thought to be characteristics of artists and not of scientists, but this is an error. Art may be defined broadly as a human activity whose purpose is the attainment of pleasure by the artist and the fulfilment of his emotional desires. The true scientist, who is not a mere technician or investigator of details, is an artist in this general sense. Imagination has been an important feature of the work of the greatest scientists. Newton's law of gravitation is as much a work of creative genius as any of Beethoven's symphonies are, although it was put forth as a universal law, and has had abundant verification as such. As Havelock Ellis says in his chapter on "The Art of Thinking" in "The Dance of Life":

> Science is not the accumulation of knowledge in the sense of piling up isolated facts, but the active organization of knowledge, . . . this task is impossible without the widest range of vision and most restless fertility of imagination.

There is a close connection between ethics and esthetics. Esthetics, usually defined as the study of beauty, is largely a study of pleasure, since beauty is that quality which we attribute to an object as the cause of the emotion of pleasure the object gives us. Birkhoff has developed a theory of esthetic measure which seems to be a promising beginning in this field. His theory assumes that esthetic value is associated with order or the degree of "unity-in-multiplicity." He limits himself to "formal elements of order, in contradistinction to connotative elements of order," but the reasons for pleasure being caused by perceiving elements of order are the same in both cases. The connotative elements can not be taken account of by a formalistic theory but are very important. The fact that we all find delight in familiar and habitual things, though too frequent repetition becomes irritating, has a physical basis that would be difficult to explain in detail. We also delight in the new if it is not completely new, and if there are some familiar features recognizable. The pleasure we feel in the recognition of old elements in new combinations may be considered as pleasure in performing the normal function of thinking, for the recognition of similarities is one of the most important and most fundamental types of thinking.

A person may be said to be impelled in the direction of maximum pleasure in a way analogous to the acceleration of a free mass in the direction of the maximum gradient of the gravitational potential field. Of course the psycholog-

ical problem is not as simple as the analogous mechanical one. While all masses placed at the same point in a gravitational field will be accelerated in the same direction, different persons in the same situation can act in different ways. The explanation in terms of the principle of maximum pleasure is that different people, because of their differing natures, give different values to the pleasures to be derived from the various modes of action possible in a given situation. In this way the maximum pleasure is obtained in different directions by different persons. The gravitational case would be similar to the psychological one if different masses were polarized in some way such that the components of the gravitational field were given different relative weights. A like case is that of an electron in a magnetic field. Electrons moving in different directions at the same point of the field will experience forces acting in different directions. Here the difference in the directions of motion of electrons corresponds to the difference in the psychological natures of individuals. The physical case is still much simpler than the psychological one, since the electrons vary only in velocity, a quantity having three independent components, while human beings vary in innumerable distinct ways. A person for whom the greatest pleasure is found in association with people and in the feeling of power that comes from being a person in authority will attempt to become a leader in business, politics or other fields. One for whom the greatest pleasure is the self-satisfaction that comes from doing apparently unselfish things will become a charitable person. And one, such as I think I am, for whom the greatest pleasure is found in learning and in understanding will naturally become a student and a scientist.

Although altruism, strictly speaking, does not exist, this concept is a convenient fiction which is often useful in the concise description of human actions. Actions which are apparently altruistic or unselfish, and usually pass for altruism, have always appeared to me, on close study, to be efforts to satisfy some obscure desire for self-approval. The person in question is often unaware of the nature of his feeling of self-satisfaction, and he sincerely believes that he is doing an unselfish act. This ability to obtain pleasure from a superficially unselfish act, which is usually the result of religious training that assumed the existence of genuine altruism, is one of the things that enable fundamentally selfish people to live together more or less harmoniously in a society where immediate selfish interests often conflict. On a higher, as least more abstract, level is an understanding of the fact that to obtain maximum pleasure from civilized life it is necessary to curb one's immediate selfish interests in favor of higher selfish interests which advance the welfare of society, and incidentally advance one's own welfare. The hope of the world for a more perfect civilization lies more in the growth of enlightened selfishness than in a Kingdom of God or in any other hazy idea of a prophet.

What, then, is good and evil, if unselfishness is not good and selfishness is not evil? We can not define absolute and universal good and evil; we can only define individual and relative goods and evils, each completely valid only for one person at one time, although with some modification it may serve at another time or place. That which is good for me now, for instance, is that which helps me to grow, physically and mentally. In general, the same is true for others, though the details of what is good for them will not be the same as for me. In so far as I have developed beyond a mere animal, my needs will be broader and my idea of good will be higher than the satisfaction of animal instincts. Is

it higher because I have a greater range of experience and desires? Or because I realize that to live happily in an organized society I must consider the sometimes conflicting needs of others, and must restrain some self-centered desires in order to satisfy desires for social pleasures? Or because I can see forward a little into the future, and can restrain a desire for immediate pleasure in order to obtain a greater future happiness? We can go no farther than this, I think, in trying to set an absolute standard of good and evil. Each one of us has a set of more or less well-defined ideas of good and evil. It is questionable whether a single concept of goodness can be formed consistent with all these ideas, or even representative of a common fundamental notion in them. The greatest good of the greatest number is commonly considered a more admirable ideal than personal happiness. But why? Is there anything to the welfare of society beyond the welfare of its members? The state itself can not enjoy anything, not even the happiness of its citizens. I disagree violently with those nationalist orators who imply that the state or nation has a life or consciousness of its own superior to that of the individuals who compose it.

There is no religion; there are only religions. By that I mean that it is impossible to frame a single definition of religion consistent with all the religions that have been developed in the history of the human race, since that which has been called religion has varied too much with person, time and place. However, most religions fall into two classes which overlap somewhat. First there are those involving a belief in a divine Being or Beings having material existence and supernatural powers. These largely consist of a primitive cosmology colored with such emotions as reverence, awe and guilt. The priests, prophets and medicine men who created the primitive religions were the scientists of their day, although the cosmologies contained in their religions are incompatible with the results of modern physics and astronomy. In fact, a supernaturalism which postulates the existence of a divine being not subject to the laws governing the rest of the world is based on an essentially unscientific attitude toward the world. A supernatural religion thus conflicts with science, even though its adherents try to avoid the conflict by saying that religion deals only with spiritual things while science deals with material things. Second there are those religions in which the God has become an abstraction without personal existence, and in which emphasis is laid on the relations of people to the others among whom they live. Some persons would call this type of religion a code of ethics, and reserve the name religion for the first class. Even religions of this second class are in conflict with science to the extent that they are based on unscientific ideas of sin and salvation.

A religion concerned only with spiritual things, such as many people claim to have, is impossible, or at least futile. Exponents of such a religion do not clearly realize what they mean by "spiritual." They imagine that human thoughts and desires are purely spiritual processes, processes that have no physical basis, because they are not aware of the physical mechanism underlying mental processes. Although no one knows the details of the physical basis of thinking, the absurdity of a belief that there is no such basis should be apparent to any one who considers the fact that if a brain is rendered inactive or damaged by drugs, accident or other physical means, the spirit or mind associated with it is invariably destroyed or greatly changed. If physical acts are really negligible to a religion, and only spiritual things are important,

then the religion becomes useless and academic, concerned only with unobservable things. Actually, religionists are always much concerned with human actions, which are physical phenomena if nothing else.

Parallel with the evolution of religions has been the evolution of ideas of God, beginning with material and personal gods possessing all the human desires and emotions of their worshippers, and progressing up to supposedly immaterial beings having none of the qualities of which their human creators were ashamed, yet still retaining the ability to know and plan and act as human beings do. The final stage in the transformation of the idea of God under the influence of science is a supreme world-spirit, immaterial and spiritual in nature, without power to act directly on the physical world. The existence of such a supernatural being is a possibility that can not be disproved; but it is meaningless and useless, since, by definition, it can never be observed. I know of no higher power than human intelligence, which has created all religions and ideas of God.

The soul, as usually imagined, is as supernatural as God and as hard for a true physicist to believe in. While I do not think that I am merely a haphazard collection of lifeless molecules, whatever I am, or my mind is, more than that clearly seems to be the result of the combination and organization of molecules. This result of combination is not matter in the ordinary sense of something having mass, but it is material in the sense that it can produce physical effects, and it is affected by physical conditions in the body. The fact that neither I nor any one else yet knows how consciousness results from the combination of molecules into cells and tissues is not a valid argument that it can not so result, or even that we can never know how.

An important role in many religions is played by the concept of sin. This is a fiction whose utility is limited by its being based on a belief in standards of good and evil that are thought to be divinely ordained and absolute, but are neither divine nor absolute, being traditional conclusions from old and very imperfect theories of human motivation and behavior. While I know of no theology in which ignorance is a sin, all the so-called sinful acts I have observed have been merely the result of ignorance or limited understanding. Even a criminal act performed under the influence of violent emotions is the result of the inadequacy of a mind, because of incomplete development, for performing its natural function of directing the body with which it is connected.

Acceptance of the ideas of sin and guilt requires a belief in a free soul with a complete knowledge of good and evil, and with the power to choose between them and either resist or yield to temptations to sin. Since I have rejected the usual ideas of the soul and sin it may be expected that I also reject the idea of "free will." This is true as far as ultimate belief in its existence is concerned, but the concept of a free will is a very useful one which need not be abandoned merely because it is false. Though usually based on a mistaken idea, free will is one of our most valuable fictions, one that is useful for the concise description of the process by which we make the multitude of decisions necessary in everyday life. Nearly the same idea is expressed by Planck in "The Philosophy of Physics": "Our consciousness . . . assures us that free will is supreme. Yet . . . we might say that looked at from outside (objectively) the will is causally determined, and that looked at from inside (subjectively) it is free." The more one's decisions are made by a free will which is the result of rational thought and of clear ideas of one's desires and of the consequences of one's

actions, rather than by an emotional free will, the more wise and intelligent one can claim to be and the happier one should succeed in being.

That every human action is a physical phenomenon determined by present and, indirectly, by past physical conditions is a conclusion that becomes more and more certain as one studies the structure of the human body and its behavior under varying conditions as a problem in physics. Here I am using physics in the general sense, the science of matter and energy and their transformations, to include all natural sciences such as chemistry and biology. Determinism seems unquestionable, and the existence of a free will, an immaterial soul or spirit which can make decisions independent of physical conditions, seems impossible when one studies the process of making decisions and attempts to analyze some typical elementary choices, and finds that apparently the choices depend on present conditions and past history. In my own case, the only one of which I have full and immediate knowledge, I have many times made as impersonal and as scientific an analysis as possible of my choices. Each time I was forced to the conclusion that the decision was determined by the situation I faced, and by my condition at the time, which condition was the result of previous experience. When I have tried to find out why I made a particular choice I have been able to see memories of past experiences which inclined me one way or the other, usually some each way, so that the decision was the result of conflicting forces, with the strongest finally winning out. By "forces" I do not mean the usual mechanical forces of physics but analogous fictions which, loosely speaking, seem to cause psychological phenomena in the same way that mechanical forces seem to cause mechanical phenomena.

To describe one of even the simplest of these forces in terms of the structure and connections of the nerve cells which produce the psychological phenomena would be very difficult. It would be practically impossible to describe the force in terms of the ultimate atomic structure of the body cells by means of some fundamental physical law such as Dirac's wave equation for the electron, generalized to apply to all other fundamental particles such as protons and neutrons. Yet I have faith that such an ultimate physical explanation is possible in principle, and is merely too complicated to carry out at present. The word faith is appropriate here since my belief in the possibility of ultimate detailed explanation lacks proof by actual performance, although it is upheld by increasingly detailed explanations that have been carried out. Faith, I would say, is belief in something unproven or unprovable that gives the believer pleasure. Beliefs in God, the soul and immortality are good examples of common faiths. Though I do not possess these faiths, I do have others, and do not mean to imply that faith is to be avoided entirely. For instance, one of my faiths is the fundamental belief of the physical scientist that the universe is orderly and understandable. Although this can never be completely proven, it is made plausible by the fact that many events and processes in nature have been found to be orderly and understandable, and it gives me great pleasure to believe it, the pleasure of the anticipation of the knowledge and power to be gained by studying the universe. In the same way, contemplation of the possibility of explaining all physical phenomena, even those amazingly complex ones called vital phenomena, by means of a single unified theory gives me a wonderfully pleasant feeling of the anticipation of power.

The problem of free will and choice becomes clearer if one approaches it sci-

entifically, trying to find the best explanation for a typical occurrence such as two persons meeting with the same situation, or situations which are practically identical as far as essential conditions are concerned, but reacting differently. Three types of explanation may be offered: First, the choice of reaction is absolutely unpredictable, so that no amount of knowledge of the situation or of the people would make it possible to predict the outcome. Second, the choice is determined by the free will or arbitrary choice of the persons involved. Third, the choice is determined by the physical conditions characteristic of the situation, and by the relation of these conditions to the physical structure of the person meeting the situation, which structure is the result of previous physical conditions. In more familiar terms, the choice is determined by the character of the person and the way the situation looks to him in the light of his past experience. The first, which hardly deserves the name explanation, denies without reason the applicability of science to human actions. The second is quite simple and does not obviously conflict with a scientific study of human behavior, but requires the observed phenomena constituting human actions to be separated from all other observed phenomena, and to be treated by an entirely different method which is rendered unsatisfactory by the admission of an unmeasurable factor that is observable only by means of effects produced without apparent relation to any past or present circumstance.

Some people who have faith in this second way of explaining a choice between alternative courses of action, but do not realize the full implications of such an attitude, have tried to rationalize it, and give themselves an opening to bring in free will, by seizing on the Heisenberg principle of uncertainty. They do not know what kind of uncertainty the principle deals with, yet they imply that even the physicist has given up determinism, apparently because uncertainty sounds as if it is incompatible with determinism. Heisenberg's principle does not conflict with a deterministic view of physics. It simply states that conjugate dynamical variables can not be measured simultaneously with unlimited precision. This bars the possibility of measuring the exact present condition of the universe, and then calculating its future conditions from the results of the measurements, even if the rules of calculation were known. But it does not deny a causal connection between the present and future. The Heisenberg principle is merely a precise statement of the observed fact that there is an unavoidable interaction between the observing apparatus and the object observed.

Probably the best example of an elementary process to which the uncertainty principle applies is the collision of an electron with a photon, called a Compton collision. Many such collisions have been observed in cloud chambers; in every thoroughly investigated case, energy and momentum were found to be conserved within the limits of accuracy of observation. Nothing is more typical of deterministic law than the laws of the conservation of momentum and energy. These laws relate fundamental physical properties of a system at one time to those at another time in the simplest possible way, by the relation of equality.

Though it is extremely important in individual quantum mechanical processes, the uncertainty principle becomes unimportant in the consideration of processes involving large numbers of atoms, as most bodily processes do. When large numbers are involved statistical laws apply with great accuracy. The small discrepancies between the re-

sults of observation and the predictions of statistical law leave little room for freedom of choice.

Closely related to free will is another useful fiction—self-control. Obviously a man can not really control himself any more than he can lift himself by his bootstraps. The idea is logically self-contradictory, but it often furnishes a convenient abbreviated way of describing an important psychological phenomenon. The common expression, "a struggle between higher and lower selves," is somewhat more accurate than self-control, but it assumes that higher and lower are absolutely defined, and it is oversimplified in that it neglects the fact that the splitting into two selves takes place in different ways on different issues. The exercise of self-control in the usual situation requiring a decision is more accurately described as follows: A man having a complex mental nature resulting from years of development and experience is confronted with a situation in which he sees two fairly distinct alternative courses to pursue. Some parts of his mind—psychological forces or whatever you wish to call them —tend to produce the choice of one of the alternatives, while other parts of the same mind tend the other way. In the case of a situation involving a moral issue, one of the courses is called yielding to temptation. The other is called resisting temptation, and the person choosing it is said to have used self-control. The psychological mechanism operating is the same in a case involving no moral issue, say the choice of the color of a hat, as in a case with a moral issue, although the strength of the conflicting forces may be much less. The choice that is made in any particular case depends, exactly as one should expect, both upon the nature of the man and of the situation in which he finds himself.

The development of self-control is then nothing but mental development and training which strengthens that side of the mind that tends in the general direction of resisting temptation. Though the faculty of self-control is one of our highest faculties, ranking with understanding and intelligence, its development by a long process of adaptation to environment is an essentially selfish process, as adaptation always is. It is generally considered that a well-developed faculty of self-control is necessary for a well-balanced mind. With this I agree, though I do not believe in self-control as most people conceive of it. I try to use as much self-control as possible, because I believe that I will enjoy life better and on a higher level if I can be as much as possible conscious of what I am doing, and for what purposes I am doing it, and choose my course of action with regard for others and for the future, rather than let habit and thoughtless emotion be my guide. This is not to imply that emotion is to be eliminated in favor of reason. Though we may try to live *by* reason, it is always emotion that we live *for*. Reasoning should be used to distinguish between good and bad emotions, between those, on one hand, that are short-sighted or low or poorly developed and those, on the other hand, that are far-sighted or high or well developed and that do not conflict with the happiness of others. Reason is the means; emotion is the end.

The concept of self-control may be somewhat clarified by comparing it with devices for controlling physical quantities such as temperature or electrical potential. There are three parts essential to a controlling device: First, a standard to which the variable can be compared, directly or indirectly; second, a means for detecting deviations of the variable from the standard; third, a means for correcting the deviations, which is actuated by the detecting means. Self-control of conduct requires three similar

things: First, the existence of an idea of a standard of conduct or of the particular phase of conduct to be controlled; second, the ability to perceive deviations of conduct from the standard; third, the ability to make corrective changes in conduct in accordance with the perceived deviations.

I do not think that I have proved anything in the foregoing paragraphs, chiefly because proof, as most people conceive of it, is impossible. Matters of objective fact have their accuracy limited by the errors of measurement, while a conclusion involving an abstract idea has its certainty limited by the limited clarity of conception of the ideas involved. To prove an abstract theorem to another, one can only indicate to him the processes by which one became convinced of its truth, and hope that he will see the truth. Whether or not he does see it, or thinks he sees it, will depend on many factors in his previous experience as well as on the absolute truth one attributes to the theorem. My feeling is that no truth exists in the conclusion of a theorem that does not exist implicitly in the definitions of the ideas involved. When I derive a proof I am only completing, by exploring their implications, the definitions of the ideas with which I started. I see no self-evident truth in axioms or postulates. They are merely disguised definitions which describe properties of the entities we have invented. Though there is no absolute standard of truth any more than there is an absolute standard of good and evil, there are two types of relative truth that can be recognized. One is internal self-consistency, such as a physical theory or a branch of mathematics may possess. The other is external consistency, such as the agreement of the predictions from a theory with the results of observation. A critical analysis thus seems to leave nothing of truth except lack of contradiction, yet the concept of truth is one of our noblest and most useful fictions, one that we could hardly dispense with. A theorem may then be said to be true if its conclusion is consistent with and logically following from its premises. In any particular case, this consistency and logical connection must be judged according to the personal standards of the person desiring to know whether the theorem is true or not. Though in many cases fairly general agreement can be reached as to what is true or what is just, in the end every man's truth is his own as is every man's justice.

Although this article was written partly in answer to that of Professor Conklin, I think that I have written in the same spirit as he in tracing the natural development of the highest types of thinking, including ethical thought, from simple origins. In agreement with my general thesis I may add that I consider the writing of this essay a selfish act on my part. I have obtained pleasure from it already and hope to obtain more from it in the future.

# THE TROPICAL PLANTATION SYSTEM

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THE plantation system is a form of agriculture in the tropics of great economic, social and political interest. In order to characterize this form of economy, we must answer three questions: What are plantations? What is their geographical distribution? What are the origins of the plantation system?

A plantation is not only an agricultural undertaking; it is also an industrial enterprise. It not only produces agricultural products; it also prepares them and makes them fit for transportation. This it must do, for it does not produce for its own needs, as does the native, but for the market and especially for the market of the temperate zones. These markets, however, are remote from the tropics, and, moreover, in order to reach them the ships have to pass through the hot, humid tropical latitudes.

In regard to the distribution of the plantation system, we note that it is found only in the tropics and the subtropics; they have long and in parts uninterrupted growing periods for vegetation, during which they produce certain valuable agricultural products that are lacking in the temperate zones. A great demand for these products, however, does not lead necessarily to the plantation system. In the Asiatic tropics, for example, spices have been produced for centuries by native peasants and have been taken by foreign traders (Chinese and Arabs) to the markets of the Far East and of the Occident. And to-day other products, such as cotton, kapok and copra, are produced either exclusively or preponderantly by the natives of the Netherlands East Indies for foreign markets. These products, which do not require difficult preparation and which can be easily transported, do not require the plantation system for their successful production.

On the other hand, the natives of the Netherlands East Indies produce only a scant one per cent. of the exported *sugar,* although they grow sugar-cane for their own needs. They use, however, either the fresh sap of the cane or make a brown, sirup-like mass which can not be transported but must be consumed on the spot. The natives are not capable of producing solid brown or white sugar, for to do this they would need, besides the sugar-cane, capital for constructing costly special sugar mills and they would have to have highly scientific and technical knowledge to operate them.

The plantation system, therefore, is found only in the tropics because it is there that crops are grown that require not only much unskilled labor but also highly technical knowledge and last investments in processing plants and equipment to prepare the products for shipment to distant markets. The result is that the natives must fit into a strange industrial order.

This industrialization is especially necessary in the cultivation of sugar-cane (and of sugar-beet in the temperate zones), because the easily perishable juice must be transformed into a product of stone-like hardness, the sweet salt, as the natives say. Other tropical plants—such as coffee, cocoa, tea, cinchona, cotton, sisal and rubber—require similar industrial processes, especially when a product of high value is to be produced. Industrialization, so unsuited by its very nature to the agriculture of the temperate zones, is therefore the most important characteristic of the plantation system.

Division of labor and a one-crop

economy go hand in hand with agricultural industrialization. It is well known that most plantations raise only one crop, such as sugar-cane or coffee or sisal, because each of these products requires its own special machinery. Rotation of crops is, therefore, impossible even in the growing of annual plants. As a consequence the soils are rapidly exhausted and new ones have to be continually prepared for cultivation.

This very one-sided economic system, which we call monoculture, results in great instability and a sensitivity to crises. Climatic changes, plant diseases, political troubles, new technical inventions and above all the market prices interfere gravely with the life of a plantation. It is thus understandable that some plantation areas have changed their products and their mechanical installations at frequent intervals. In the nineteenth century Ceylon, for example, produced successively cinnamon, coffee, cinchona, tea and rubber. Similar changes took place at the end of the eighteenth and at the beginning of the nineteenth century in the West Indies.

The same unrest and instability are manifested in the migration of plantation products. It is sufficient to call to mind the spread of coffee culture from Abyssinia to Arabia and southeast Asia, and then to the New World, where there was considerable shifting of coffee cultivation within the American tropics, and finally to the recent completion of the circle back in Africa.

Since the installation of expensive machines pays only if production is on a large scale, it follows that the plantations are almost always large estates of several hundred to several thousand acres. These large areas require, as do the associated factories, a great number of laborers. The labor problem is thus of paramount importance to the plantations, and this demand for plantation laborers was fundamentally responsible for the former Negro slave trade, as well as for to-day's great labor migration within the Asiatic tropics. Finally, the management of the fields and factories, and the sale of the products as well, must be in the hands of trained specialists. Since as a rule natives lack training and experience, it is generally true that only Europeans (in the cultural sense of that word) are fitted to be managers of plantation enterprises.

A plantation is, therefore, a large agricultural and industrial enterprise, managed as a rule by Europeans, which, at great expense of labor and capital, raises highly valuable agricultural products for the world market.

Turning to the question of the origin of this very special type of economy, it is not surprising to learn that it is closely connected with the fabrication of solid white sugar. Karl Ritter, the great German geographer, reached this conclusion a century ago, but his results have been forgotten even by German geographers. According to this famous scholar, the refining of sugar was invented in the seventh or eighth century A.D. in the Persian province of Chusistan (in the lower course of the Tigris and Euphrates Rivers), where European-Oriental science came into direct contact with the production of tropical sugar-cane. From the beginning sugar refining has gone hand in hand with the plantation system, and both have had a spectacular migration around the earth within the tropical and subtropical zones.

The Arabs laid out sugar plantations in the Mediterranean area, from them the Venetians and Genoese learned the science and the art of making sugar, and from them, in turn, the Spaniards and Portuguese. The two last nations carried the oriental type of agriculture and the Asiatic plant to the West African islands of Madeira and the Canaries, and from here it was taken to the tropics, and found its first classic tropical develop-

ment, in the closing years of the fifteenth century, on the small Portuguese island of Saint Thomé, in the inner Gulf of Guinea. In 1492, when Columbus started on his great discoveries, the sugar plantation system was well established on this island.

But these small West African islands soon lost their significance as sugar-producing centers when sugar-cane cultivation, together with the plantation system, was extended to the New World: to Santo Domingo in 1519 and to Brazil in 1531. Here much larger areas suitable for sugar-cane cultivation were available, and, in addition, the cane did not have to be irrigated, as it did in Madeira, the Canaries and the Mediterranean areas. Therefore, in spite of the greater distance of America from the European market, its sugar could be sold much cheaper, as is evidenced by the rapid fall of sugar prices in the sixteenth century.

The capital needed for the American sugar plantations was supplied by merchants of Lisbon (apparently many Jews), and by nobles who had acquired wealth in the spice trade of East India. Only laborers were scarce or even entirely lacking, but this problem was solved in an ingenious but cruel manner by the importation of African Negro slaves. Thus every continent had a share in the rise of the plantation system in the New World: Europe furnished the capital, Asia the sugar-cane, Africa the laborers, and the Americans the climate and soil.

The plantation system as we know it to-day had its first development in the American tropics. Here also for the first time crops other than sugar-cane were raised under this type of economy: indigenous tobacco, cotton, cocoa and, most surprisingly, in the middle of the eighteenth century, African coffee. In the early days small and middle-sized holdings often developed near large plantations to grow these new crops, but our knowledge of these types of agricultural economy is very limited. Until the beginning of the nineteenth century all these types of enterprises were found only along the coast of Brazil and in the French and British West Indian Islands.

The Negro revolt in French Haiti in 1789 and the abolition of slavery in the English colonies in 1833 shook the plantation system, which until that time had been very stable, to its very roots and caused a new migration. Then for the first time the plantation system reached very significant proportions on the Spanish islands of Cuba and Puerto Rico, spread to the continent, and developed in Venezuela, Colombia and Central America, where indigo and, even more important, coffee were the chief products. Coffee plantations now also began to migrate through Brazil.

Much more extensive in area, however, and economically more important, was a kind of retrograde shifting of the plantation system from America over Africa to Asia, whence its migration had begun a thousand years earlier. The rise of steamship transportation and the later opening of the Suez Canal favored the development of these new plantation areas, as did the continuation of slavery in tropical Africa until 1880 and the availability of a large number of cheap laborers in tropical Asia.

In tropical Africa, which is very difficult of access and is inhabited by free Negroes who resist attempts to force them to work for wages, the plantation system is still unimportant. Only on the islands of Saint Thomé, Mauritius and Réunion has the plantation system attained great significance since 1830.

In the islands and peninsulas of tropical Asia, however, the plantation system has become the predominant type of economy. Here even those plants which in other tropical regions were simply acquired by a gathering economy are

raised on plantations; among these are cinchona, rubber and recently the African oil-palm. The transferring of cultivated plants from one continent to the other required great expenditures of money for scientific research, especially for plant-breeding and seed selection. The private entrepreneur was not fitted for this purpose, and therefore in the Asiatic tropics the plantation system was developed by joint stock companies. In the same manner and with the same success the newest branch of tropical American plantations—the banana culture—was built up for the market of the United States on a large-scale capitalistic basis. The capital here is needed not for the cultivation or preparation of the product, but for its transportation in special freighters.

The economic and social life of the tropics has been widely influenced by the plantation system. The Europeans brought capital and knowledge, the countries contributed soil and the natives labor. In this process the natives were often deprived of their land, uprooted from their social environment and transformed into a landless proletariat, despite the abundance of land in the tropics. Therefore, many people condem the plantation system *per se* and propose to leave the entire production of tropical goods to the natives. Besides these social and economic aspects, more and more ethical considerations are advanced by the opponents of the plantation system. The welfare and progress of the natives, in their opinion, should be the only, or at least the dominant objective of colonial policy. Under no conditions, they say, should the natives be deprived of their land, because only on their own soil do they have the opportunity of preserving their national life.

In opposition to this ethical conception the adherents of the plantation system offer economic arguments. The natives, they point out, because of their primitive economic methods are not fitted to produce all tropical goods so necessary for the inhabitants of the higher latitudes. Such products as sugar, sisal, quinine, etc., which require industrial preparation, can be grown only by Europeans (the word used in its cultural sense) and not by the natives. Even in growing products which can be easily prepared, such as tobacco, coffee, cocoa, tea, etc., the natives are far behind the European plantations in the quality of their products. Only in the growing of annual plants, such as cereals, groundnuts, cotton, etc., which require little or no preparation, are the natives superior to the Europeans. It is not simply a question of plantation system or peasant economy: both types are, in the opinion of these proponents, necessary for the development of the tropics.

Keeping in mind these two points of view, others are pleading for a collaboration between Europeans and natives on the basis of equal rights and equal duties. To the common production, the native should contribute his land and his labor, and the European his capital and his technique. The returns should be divided between the two partners according to certain principles. Unfortunately, the application of this very simple and obvious proposal is almost impossible because of the fact that both partners are very different in racial, cultural and social characteristics, that most natives lack the moral basis for such a collaboration and most Europeans the social will for it. Only under economic pressure has such a collaboration thus far succeeded, notably in the cultivation of sugar-cane, which is relatively simple to grow but very difficult to prepare. In the Fiji Islands, Mauritius, the West Indies and Brazil to-day the natives raise the cane and sell it to the whites, who process it in large central factories. This procedure has the ethical disadvantage that the natives are, for the

most part, not the owners of the land, but only tenants who can be discharged at any time if they do not fulfil their duties.

To remedy the defects of the present policy, English colonial politicians are pleading for another system that will do justice to the natives as well as to the economic interests of the Europeans. Under this proposal the state would mediate between the independent peasant and the white entrepreneur, regulating by legislation the rights of the Europeans and the duties of the natives. This principle, called by M. Leake "triple partnership," found its first practical application in the cotton cultivation of Gezirah (Egyptian Sudan). There the European employer is represented by the Sudan Plantations Syndicate, which is a kind of Chartered Company but without rights on the land. The land belongs to the natives, who are required to till it in a precisely prescribed manner. The syndicate processes the cotton and carries out the irrigation and all commercial activities under the regulations, the barrage and the main canals having been built by the state. The profit is divided in equal parts among the three partners.

This principle seems to be suited especially to dry regions (Indus, Niger), where expensive projects are required for irrigation. But even in those humid regions of the tropics where large areas are not opened and must be developed by means of communication, this principle should be successful, because it combines the advantage of the plantation system with that of the peasant economy and avoids as much as possible the disadvantages of both. This, of course, supposes that Europeans and natives are psychologically prepared for such collaboration and that there is an enterprising state to guarantee its legal foundation.

# THE RACE CONCEPT IN BIOLOGY

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The perennial discussion of the nature of races, particularly of those in man, has become especially lively, frequently acrimonious and notoriously inconclusive during the last decade. Although the problem obviously is in part a biological one, biologists have, with few exceptions, disdained to take part in the debate. An apparently good reason for this forbearance is that the debate on the "race problem" is not conducted on a scientific plane at all. Yet biologists can not escape a part of the blame for the disrepute in which the race problem has fallen. The plain fact is that in biology itself no clear definition of what constitutes a race has been evolved. The existing concepts are either fundamentally unsound or so ambiguous as to be of little use for rigorous thinking. The refined analytical methods of modern genetics may permit a better insight into this problem to be gained than was possible in the past, but the work in this field is now barely begun. The purpose of the present article is to outline the salient features of the situation.

Most taxonomists and anthropologists cling perforce to the habit of describing races in terms of averages of morphological and sometimes of physiological and psychological characters. We are told that the average Eskimo has such-and-such a height, cephalic index and intelligence quotient, while different sets of figures are given for the average German or Hottentot. This method is, because of its simplicity, undeniably convenient for a rough description of the observed variety of humans or of other living beings. The trouble is that it leads to a hopeless confusion when an analysis of the underlying causes of this variety is attempted.

A race defined as a system of averages or modal points is a concept that belongs to the pre-Mendelian era, when the hereditary materials were pictured as a continuum subject to a diffuse and gradual modification. Genetics has established that the hereditary material, the germ-plasm, is not a perfect continuum, but rather a sum of discrete particles, genes, which change one by one by mutation. This is no trifling distinction, and its corollaries must be appreciated. If germ-plasms could blend with each other as a water-soluble dye commingles with water, every interbreeding population would soon reach a reasonable uniformity, and every individual would in a very real sense be a child not only of its parents but of its race as well. A "pure race" would be formed in each locality occupied by the species. With the germ-plasm being particulate, the variety of genes present in a population tends to be preserved intact indefinitely; the genetic constitution of an individual does not necessarily lie midway between those of its parents; some of the genes of an individual may resemble those commonly present in the population from which it sprung, while other genes may be identical with those usually found in representatives of another race. Except in asexually reproducing organisms, pure races can be formed only under very exceptional circumstances (a long-continued inbreeding of close relatives). Since the germ-plasm is particulate, the variation within a population can adequately be described only in terms of

the frequencies of the variable gene alleles and of their combinations. Differences between populations must likewise be stated in terms of the differences in the frequencies of genes present in them.

A geneticist can define races as populations that differ from each other in the frequencies of certain genes. The obvious flaw in such a definition is that differences in gene frequencies may be quantitatively as well as qualitatively of diverse orders. The statement that two populations are racially distinct really conveys very little information regarding the extent of the distinction. This can be made evident by a series of examples illustrating the different degrees of racial separation. The examples given below concern mostly lower organisms, and particularly the small flies belonging to the genus Drosophila. The reader may be inclined to question the applicability of the conclusions reached through studies on this material to organisms in general, and particularly to man. Although in dealing with man the complications resulting from his social organization must not be lost sight of, the laws of heredity are the most universally valid ones among the biological regularities yet discovered. The mechanisms of inheritance in man, in the Drosophila flies, in plants and even in the unicelluars are fundamentally the same. The race concept is very widely applicable, at least among the sexually reproducing forms of both the animal and the plant kingdoms. It can be elucidated most effectively through use of a favorable material, which is, for technical reasons, readily amenable to the application of the experimental and quantitative methods of modern genetics.

The fly *Drosophila pseudoobscura* is a species widely distributed in western North America. Although its representatives from any part of its geographic range appear to be similar externally, genetic analysis reveals a considerable variability under the guise of external uniformity. The variability concerns both the gene arrangement and the gene contents of the chromosomes. Genic variability is displayed in the occurrence of mutant genes that affect the external structures, viability, development rate and other characters. Most of the mutants are recessive to the "normal" condition, and rare enough so that only heterozygotes occur in natural populations.

None of the populations of *Drosophila pseudoobscura* so far examined proved genetically uniform; in every one of them some individuals carried chromosome structures and mutant genes not present in others. Every population may be characterized by the incidence of the genetic variants present in it. Comparison of populations from different localities usually shows them to be unlike, since some of the genetic variants present in one either do not occur at all or occur with different frequencies in others. It is astonishing that even contiguous localities may harbor different populations. In forms which can move only very slowly, such as land snails, differences of this kind have been known for many years. Yet similar differences are observed in the much more mobile Drosophila. In one instance a statistically significant "racial" difference has been observed between populations of localities about 100 meters apart, although the intervening terrain contains no obvious barriers that could impede the migration of the flies. Mobility of an individual organism does not always prevent an extremely fine subdivision of the population of a species into local races. Studies of Dahlberg and others suggest that such a subdivision may occur also in man, since the incidence of certain genes may be different in populations of neighboring villages.

More unexpected still is the fact that

the genetic composition of a population of *Drosophila pseudoobscura* does not remain constant with time. In certain populations from California the incidence of various chromosome structures has been observed to change not only from year to year but from month to month. The causes of such alterations are as yet not clear. The most probable conjecture is that the food sources are unevenly distributed in the territory inhabited by the flies, and that a single or a few individuals which first reach and monopolize an abundant food supply leave an offspring large enough to impress their individual characteristics on the population of the surrounding area. As shown by Sewall Wright on basis of theoretical considerations, both temporal changes and a gradual drifting apart of the genetic composition of local colonies are expected to occur where the effective sizes of local populations are limited. It seems, then, that local populations may be effectively small even in species possessing as good locomotion means as Drosophila. Perhaps an analogous situation in man is the occurrence of villages in which some family name, the heritage of a prolific early settler, is much more frequent than in the population of the surrounding territory. However that may be, changes in the racial composition of local populations may be observed in nature well within a human lifetime. Evolutionary changes in nature are not too slow to be observed directly.

The drifting apart and the consequent racial differentiation of local populations is a process which, by itself, can not be regarded as an adaptation to the environment. It is rather the forerunner of an adaptive differentiation. Genetic changes which arise in a species are subject to natural selection which eliminates the unfit and preserves the valuable variants and the populations in which such variants become frequent. Since the environment is seldom uniform throughout the distribution area, the species differentiates into local races that are adjusted each to the environment prevailing in its particular habitat. Such local races, termed by Turesson ecotypes, do not, as a rule, form continuous populations over large parts of the species area. They recur wherever the proper environment is available, while the intervening localities are occupied by different ecotypes or not inhabited by the species at all. Ecotypic differentiation has been described in many plants by Turesson, J. Clausen, Gregor and others; among animals this phenomenon seems less wide-spread, possibly because the mobility of most animals makes them less dependent than the plants are upon the micro-environment of their habitat. Nevertheless, Dice and Blossom have shown that in the mammals of the North American desert the coat color becomes darker or lighter, depending upon the prevailing shade of the soil on which they live. Dark ecotypes occur on the outcroppings of lava, and light ones on stretches of light sand. An important fact is that ecotypic differentiation does not, as a rule, involve the entire mass of individuals residing in any particular habitat. Thus, the light average shade of the coat color in mammals inhabiting light sand is due merely to a greater frequency of lightly colored specimens in sandy localities, although the darkest individuals on light soil may be much darker than the lightest ones on dark soil.

While the exigencies of adaptation to the strictly local and recurring conditions of the habitat lead to the formation of ecotypes, adaptation to more general variations in the environment results in formation of geographic races (otherwise known as subspecies or ecospecies) which occupy more or less continuously definite parts of the species area. Taxonomists are well aware of

the fact that the differences between geographic races are slight in some cases and much more striking in others. Since the environment changes more or less gradually as one passes from one region to another, the changes in the appearance of the species population may be correspondingly gradual. Where a definite geographic boundary between races is discernible, the races are nevertheless found to merge into each other in at least a narrow boundary zone. This situation is described by taxonomists as "overlapping" or as presence of intermediates between the races. This is a very misleading way of stating the observed facts, for it implies the notion of "pure races" the intermediates between which are sometimes formed. It is more accurate to say that the frequencies of the variable genes change more or less gradually or abruptly during the passage from one portion of the species area to another. If the characters distinguishing races are examined one by one, geographically graded series or, to use the term recently proposed by Huxley, "clines" are encountered. The clines in gene frequencies are what cause the appearance of clines in the outwardly visible characters. The naive concept of pure races connected by intermediates must be replaced by the more authentic one of the varying incidence of definite genes. The idea of a pure race is not even a legitimate abstraction: it is a subterfuge used to cloak one's ignorance of the nature of the phenomenon of racial variation.

As a general rule, the further two populations are removed geographically the greater are the genetic differences between them. In *Drosophila pseudoobscura*, this rule is infringed upon chiefly where very small distances are involved, since the fluctuations in the composition of the population in any one locality (see above) may be large enough to obscure the more general geographical trends. Thus, the variations in the local populations on Mount San Jacinto, California, appear to be haphazard. The localities from which these population samples were taken are from 100 meters to about 25 kilometers apart. Populations from Mount San Jacinto and from the Death Valley region, a distance of about 400 kilometers, are difficult to distinguish if only small samples are available. The difficulty is alleviated if a number of large samples from several localities in each region are studied. Comparing the data for the eastern and the western parts of the Death Valley region, Mount San Jacinto, Mount Wilson, San Rafael Mountains and San Lucia Mountains, California, we find pronounced east to west racial clines. The populations inhabiting Texas are, however, so different from those of California that a single small sample can be determined as coming from one or the other of these regions. Nevertheless, the ability to distinguish groups of individuals, populations, does not necessarily imply that every individual may be classified as a representative of one or the other races. Thus, some individuals from Texas are identical in chromosome structure with those from California, although the Texas and California races as groups are undoubtedly distinct.

While the process of "raciation" must be regarded as predominantly an adaptive one, it does not follow that every difference in the gene frequencies is a direct result of natural selection. Some of the characteristics distinguishing races appear to be adaptively neutral. Without going into the details of this very perplexing problem, one may say that the racial subdivisions of a species are a product not only of the environment now existing but also of a

long historical process of evolutionary development. A race inhabiting a country is what it is, not only because it lives there but also because it came from a definite source, following a definite distribution path or paths.

Superior adaptive types, having originated in different parts of the species area, may spread and finally confront each other across a more or less narrow boundary zone. When this stage is reached, the races may develop isolating mechanisms that would prevent them from interbreeding and hence from exchanging genes with each other. The establishment of isolation connotes the transformation of races into separate species and is therefore outside the scope of the present article. The point which should be made clear here is that a race becomes more and more a reality, and less and less an abstraction, as it approaches the species rank. Species attain a degree of existential concreteness which makes them independent actors in the drama of life. In terms of this histrionic analogy, races of a species may be likened only to members of a choir. The prime characteristic of a species is that individuals belonging to it are prevented from interbreeding with those of other species, but not with each other, by physiological isolating mechanisms. It is, therefore, legitimate to speak of pure species (contrasting them with hybrids between species). Yet, identical gene variants continue to occur in races that are well advanced toward the species rank, as well as in separate species.

The description of the racial composition of a species in terms of the variations in gene frequencies presupposes a careful genetic analysis of the material under study. Unquestionably, this is a slow and difficult task, especially where, as in man, the conditions for genetic work are unfavorable. A satisfactory insight into the nature and significance of the racial differences in man demands far more extensive and detailed information than is now available on the mode of inheritance of the characters causing interracial, as well as intraracial, variability; scientifically controlled data on the manifestation of the diverse genetic conditions in various environments; and a thorough knowledge of the incidence of the determining genes in the class, caste and race subdivisions of the mankind. Evidently, this task can be accomplished only at the expense of concerted efforts of many scientists and organizations in different parts of the world. Yet, the difficulty of the task is not a sufficient reason to cling to the outworn methods of racial study, the inadequacy of which is quite plain, and still less is it a reason for erecting far-reaching theories on the basis of admittedly faulty data. To do so would be a travesty on science. It is said that Menaechmus warned Alexander the Great that "There is no royal road to mathematics." There is no royal road to genetics either.

# RELIGIO SCIENTIAE

By CHAUNCEY D. LEAKE

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IN the October SCIENTIFIC MONTHLY the Reverend John S. O'Conor, S. J., professor of physics, Georgetown University, asks "Why do men of science refuse to approach the subject of religion from a scientific viewpoint?" The answer most probably accurate for most of them is that they are afraid to. The fear may be not so much of the subject as of hurting the feelings of the many people who are intensely interested in it.

The purpose of this article is not so much to try to answer the Reverend Professor O'Conor's question, as it is to try to point out that his subsequent conventional argument is scientifically questionable. Many older and more considerate scientists may wonder why any one concerned with science should bother with the Reverend Professor's time-worn arguments. He relates them by analogy, however, to current physics. As a result, practical and busy scientists or laymen may feel that if the essay stands unchallenged, no logical answer is possible. While even slight sophistication in dialectic may raise prompt doubt in the casual scientific reader of the article, lack of time on the reader's part may prevent adequate analysis to justify the skepticism. I am venturing this challenge with full realization that I am plunging into philosophical wildernesses, without the qualifications to pose as a competent guide therein.

It is my feeling that the Reverend Professor O'Conor welcomes discussion of his article, for he may agree that debate may help to clarify the problem, and to stimulate more interest in it on the part of scientists. It may be wise for scientists these days to pay more attention to the general implications of their efforts. Philosophy still has a place in our intellectual affairs if we can persuade ourselves to attempt the co-ordination of our general knowledge with our common desires, as Dewey might put it. Walter Lippmann in "A Preface to Morals," New York, 1929, reminds us that this was part of the wisdom of Confucius, and still is a function of religion.

For those scientists who may be seriously interested in this matter, but who may not be familiar with readily obtainable surveys of thoughtful opinion on it, let me suggest consultation of the critical bibliographical note at the end of H. L. Mencken's "Treatise on the Gods," New York, 1930. The outstanding sources remain "The Catholic Encyclopedia," 16 vols., New York; J. Hasting's "Encyclopedia of Religion and Ethics," 13 vols., New York, 1908, and A. D. White's "History of the War of Science with Theology in Christendom," New York, 1896. An entertaining and fair summary of philosophical development is Will Durant's "The Story of Philosophy," New York, 1930, and Henry Alperin's "The March of Philosophy," New York, 1934.

## DEFINITIONS

As the Reverend Professor O'Conor points out, it is pertinent to begin our discussion by trying to define our terms in a manner acceptable to all. While he proposes a definition for religion, he neglects to give similar consideration to science.

Father O'Conor says:

the etymological or nominal definition of religion is open to two interpretations, one which is based on the notion of a bond (from the

Latin *religare*, to bind) and another which stems from the Latin derivative *relegere* or *religere*, to treat carefully, to ponder or meditate. . . . An unbiased study of the history of religions and of comparative religion sustains a position which maintains that, despite the presence of admixtures such as ancestor worship and accretions of magic and witchcraft, the notion of a supernatural or supreme being is contained at least implicitly in practically all religions. So that on the first interpretation of the nominal definition of religion the idea of God is introduced historically as the term of the bond between man and a higher being, while on the second interpretation this supreme being appears on the same historical basis, as the object of man's meditations.

While this definition conforms to the usual and conventional one, it is hardly adequate for our problem, since it begs the question and gives an answer in which the Reverend Professor O'Conor is interested. For our proposed discussion would it not be simpler to define religion more generally as the *faith* or *belief* one has with respect to the relation between one's self and one's environment? There is no logical or factual necessity of which I am aware that requires that one's faith or religion either nominally or historically must include recognition of a supernatural or a supreme being. That it usually *does* historically is no reason why it always *must*. According to the dictionaries, current usage, limited though it may be, permits the broader definition which I have proposed.

We may also follow usage, as indicated in the dictionaries, as a guide in defining science. May we assume that science is the body of agreed upon and objectively demonstrable knowledge about ourselves and our environment? If so, we may go on with St. Thomas and say that where science ends faith begins. On the basis of these definitions one might say that whereas religion is what one *does* or *wants to* believe about one's self and the universe, science deals more with what is logically and demonstrably *permissible* to believe about one's self and the universe. Science seems to be concerned chiefly with estimating the limits to faith, while dogmatic religion seems to be more concerned with trying to guide the steps of blind belief. Philosophically science tends to be materialistic, pragmatic, realistic, while the trend of religion is idealistic, intuitive, subjectivistic.

## Aim, Spirit and Method of Science and Religion

More to the point would be to examine the aim, spirit and method respectively of science and religion. If these may be harmonized, and I believe they may be, then science may become a part of religion and *vice versa*. As forms of human activity they're like the obverse and reverse of the same coin. Religion represents the subjective approach to the universe, science the objective. The difficulty may be simply that through the chicanery of word symbolism the devotees of both wish to be "heads."

In his preface to "The Direction of Human Evolution," New York, 1921, Edward Grant Conklin attempts a definition of the aim, method and spirit of science. He identifies the aim of science with that of religion, to know the "truth" about ourselves and our environment. By the abused idea symbol "truth," he means an objectively demonstrable and intellectually coherent explanation of ourselves and the universe. While appreciating the Platonic ideal of absoluteness in "truth" as desirable, the spirit of science suggests, as C. J. Herrick emphasizes[1] that scientists recognize that knowledge of ourselves and our environment is in the process of acquisition and agreement and will probably always remain so. Scientists dodge Paul Elmer More's "Demon of the Absolute," Princeton, 1928, by admitting honestly that what is called the "truth" about ourselves and the universe is tentative and subject to modification. It is not

[1] *Jour. Philosophy*, 33: 169, 1936.

clear that the spirit of religion suggests to its devotees a similar relativeness for "truth." Most religious leaders seem to consider their particular "truth" as immediately absolute, and therefore binding on every one, whether agreed to or not.

The spirit of science, unlike its aim, seems to be a little different from that of religion. Conklin suggests that the spirit of science is essentially concerned with freedom to seek the "truth." He emphasizes that the spirit of science implies not only freedom to have and to express any view for which there is rational evidence, but also recognition that knowledge of ourselves and the universe is incomplete and subject to revision, and that there is no legitimate compulsion to belief beyond the voluntary acceptance of demonstrably rational evidence.

It is not easy to try to describe the spirit of religion, but it may be inferred from the conduct of religious leaders, whether one considers the great ones of history or living preachers and churchly dignitaries. The spirit of religion seems to be blissfully naïve, and intrigued by the naturally improbable. It encourages introspection, contemplation and self-pity. It bathes in mysticism, is enthralled by symbolism and clothes itself in vagueness. It seems to be fascinated by magic and the incomprehensible, and is not disturbed by the paradox of believing in the reality of the unreal. The spirit of religion promotes prophecy, but in a retributive or intuitive manner, far removed from the sort of prediction ventured by science on the basis of calculable probability.

The spirit of religion seems to be concerned greatly with the task of persuading people, by hope or fear, to socialize their conduct. It promotes standards of behavior, and assumes an ideal ethic. Here a serious difficulty arises. Refusing to confine itself to the limits of scientific knowledge, which it rejoices to transcend, the spirit of religion roams as far as the human imagination can go. The resulting great variety of faith and belief produces continual increase in the astounding number of religious sects. Even with the common basis of agreed upon "authority" in the Bible, divergent doctrines develop to the point of scandal in Christianity, and compromise its ethics, as Niebuhr tacitly admits.[2] Lacking objective criteria or data on which agreement may be arbitrated, as in science, social harmony is difficult for religious leaders to obtain except by appeals to some "authority" operating through fear, hope or other emotional force. The spirit of religion thus seems to be reflected more in emotional reactions than in intellectual responses.

George Santayana[3] might go on to compare the spirit of religion to that of an impatient, impulsive and very attractive adolescent girl, who, when curbed by the precise and pedantic spirit of science (whom she would call "inhuman"), might petulantly cry, "You can't keep me from dreaming." The spirit of science seems to be tolerant enough to appreciate the spirit of religion, to be attracted to it as an object of study, and indeed even to love it with nostalgic remembrance of its own adolescence. On the other hand, the ever young spirit of religion, like Peter Pan, can hardly be expected to have much regard for the staid and preoccupied spirit of science beyond an uncomprehending deference. Whether a marriage between them could ever be consummated is as debatable as whether such a marriage is desirable.

As I understand the situation, a scientist can not take without protest the Reverend Professor O'Conor's definition of the method of science as being "one which accepts facts and attempts to fit them into a theory or system." This

[2] "An Interpretation of Christian Ethics," London, 1936.

[3] "Reason and Religion," New York, 1913.

definition assumes *a priori* the theory or system of thought, an assumption which may be questioned by a scientist, but not by even so liberal a philosopher as Hugh Miller.[4] The method of science seems to be more concerned with the establishment of fact. The successful scientist is usually pragmatic. He may proceed in either of two broad ways. In one he may be chiefly empirical, by observing and describing as carefully as possible some phenomenon in his environment or himself, and then go on to offer a tentative explanation of it, the validity of which he may then test by experiment, confirming or modifying his ideas in accordance with the results of such experiments. This is the way of the life sciences. In the other he may be more rationalistic. He may indeed proceed somewhat along the way suggested by the Reverend Professor O'Conor, but in reverse. He may try to build by experimental reasoning within the strict limitations of logical consistency, a coherent ideal system with which some of the details of the universe about us may be found, on experiment, to correspond. This is the mathematical way.

It seems to me that the usual method of religion, which one may trace through all history, and which one may find exemplified in the Reverend Professor O'Conor's "Approach," is to assume "God" and "immortality," and then relate everything else in experience to these assumptions. However dialectic it may be, the method of religion is basically anthropomorphic. In a Kantian sense the method of science is anthropomorphic also, but it implies deliberate discounting of psychological or emotional factors. In following the method of science, a scientist must always guard against confusing his symbols with what they may represent. In developing Aristotelian logic, religious leaders have sometimes permitted this confusion to carry them to conclusions impossible under more rigorous scientific inquiry. The method of religion seems to be directed toward the gratification of the Microcosm, while the method of science admits the limitations imposed by the Macrocosm. Religion may yet adopt the method of science, but science can not consciously adopt the historical methods of religion.

[4] "History and Science," Berkeley, 1939.

### Authority in Science

Referring to scientists the Reverend Professor O'Conor puts three subtly leading questions, which, no matter how answered categorically, might force the unwary to accept a position incompatible with the free spirit of science:

> Are they *assuming* without reason that faith and science are irreconcilable so that any attempt at reconciliation is doomed to failure from the start? Do they *postulate* without further examination that dogmatic religion is necessarily and essentially incompatible with the scientific method? Do they *deny a priori* that authority as a source of true knowledge must be abandoned *in principle?* If they do then they are no longer acting in the role of scientists but are subscribing to propositions the truth or falsity of which they show no evidence of having investigated.

To these questions, replies are ventured which are framed in the light of the definitions proposed for religion and science. First, it is my opinion that all scientists who have thoughtfully considered the matter appreciate the reciprocal relations of faith and knowledge, and that they ordinarily reconcile religion and science as far as the implications of knowledge permit. Second, I think it is clear from the attempted definitions that dogmatic religion is incompatible with the scientific method as long as dogmatic religion *requires* the postulation of a first and unproduced cause of the universe, or of the *necessary* assumption of any other idea for which direct objective evidence is lacking. When a consideration of multiple hypotheses is appropriate to a problem, science permits the postula-

tion of any idea not logically absurd or directly refutable by available evidence, but it can not permit either bias or commitment *a priori*. Science makes a special point of discounting any judgment which *may* be influenced by psychological factors of training and conditioning, or emotional factors involving wishful or fearful thinking of any sort. After rather careful examination scientists seem to have come to the conclusion that the ordinary manifestations of the methods of dogmatic religion are incompatible with those of science. Third, it seems clear from a consideration of what science is about that it could not exist were authority in itself to be acknowledged as a source of "true" knowledge. In obtaining or interpreting data in any scientific field, one of the necessary duties of a scientist is to raise doubt regarding the intellectual validity of giving more than respectful consideration to any "authority" as such, since there is always the probability that the "authority," being human, may have an axe in the matter to grind. Voluntary and uncoerced agreement among reasonable competent scholars in a field, on the basis of the objective evidence available, constitutes the only "authority" which science can recognize. Appreciation of the relativeness and tentativeness of "truth" or "knowledge" of ourselves and the universe and good-humored alertness to the human qualities of "authority" are potent factors against the development of a scientific "canon." None the less a conscious effort is also necessary on the part of scientists to prevent the growth of any "authority" which may impede the freedom of science. Sincere and pertinent skepticism remains a fundament of science and a scientist is duty bound to try to apply it as vigorously to his own data and opinions as to those of others. This discussion of the questions asked by the Reverend Professor O'Conor indicates the absurdity of the conclusion he draws from the gratuitous answer he makes to them.

The Reverend Professor O'Conor's plea for deference to "authority" in religion and science has serious implications these days. There are reciprocal relations in science and democracy which require freedom for operation. From our discussion so far it would seem that science could be employed much more satisfactorily as a curb to the roving and restless spirit of religion than an arbitrary "authority." The function of a church seems to be chiefly to exercise "authority" in religion. Historically it has been demonstrated that science can flourish only when it is independent of any such "authority." There is a peculiar danger now that "authority" may sweep democracy from the earth. It is the duty of scientists to remain conscious of this danger, and thus to resist to the utmost any attempt to foster respect in science for any arbitrary "authority" as such.

### Proof of the Existence of God

Having set the stage by leading questions, and by a definition of religion which states his conclusions, the Reverend Professor O'Conor undertakes "a *proof* of the existence of God." He begins this by stating categorically, "The position taken here is one which is entirely unassailable on anthropological grounds." Such a statement may be his opinion, but I think he will be fair enough to admit that other opinions are not only possible but probable. Since the whole of the Reverend Professor O'Conor's position is dependent upon "the proof" which he says he is going to furnish, one may be justified in expressing surprise that "the proof" is difficult to find. The "proof" offered consists chiefly of begging the question and vague analogy. Granting the Reverend Professor O'Conor's assumption of the objective existence of God, the rest of his

position follows logically and along the lines established by dogmatic religions everywhere. Its novelty consists only in its relation to current physical ideas.

But why grant his assumption? Why not acknowledge it for what it is, namely a postulate of great interest historically, and of great power traditionally. The pragmatic observation that the logical edifice erected upon it has worked and continues to work, is no proof of its correctness or that it corresponds with reality through any necessity scientific, logical or otherwise. It remains an interesting philosophical problem to guess whether or not a logical system erected upon the contradictory opposite of this assumption would not work as well.

When a problem is offered to science, on which it is difficult to obtain direct objective and measurable data, it is often approached by proposing multiple hypotheses. These possible explanations are then examined as carefully as can be with reference to all available evidence for or against them. The one having the greatest probability of coordination with acceptable "reality" is tentatively selected as a working basis for further studies. This does not preclude, of course, further investigation of such hypotheses as may have been rejected.

Many hypotheses have been proposed regarding God. Those that postulate existence of a supreme being usually resolve themselves in discussion to matters of definition. Passing by such philosophically subtle postulates as Hegel's *universal reason*, or Paulsen's *universal will*, we may illustrate our difficulty by reference to such relative opposites as are represented by common idealistic and materialistic notions of God. The traditional hypothesis, in which the majority of people seem to believe, is that God is an ideal and universally dispersed entity of omniscience and omnipotence. Rather far removed from this definition is the more sophisticated idea that God is the sum total of the capabilities of some ninety-two elements, their possible combinations and permutations and the forces that are associated with them. While the Reverend Professor O'Conor might agree to either of these definitions, he would probably insist that man has no share in developing either. On the contrary he would probably insist that "God" on either definition reveals Himself to man, in the former case through the visions of religious leaders and in the latter through the search of scientists.

There remains another sort of hypothesis. This involves the notion that man and the universe are made of the same stuff, and that what people have been calling God is as much themselves as not themselves. This implies that the ancient dualism of "mind and matter," or of "self and non-self," is a postulate of faith and has no correspondence with reality. From this position the question of the objective existence or non-existence of God becomes irrelevant and immaterial. Perhaps this is the implication of Kant's famous antimonies. *In the calculus of science, reality is as asymptotic to the idea of a first and unproduced cause as it is to the idea of infinity.* This statement is one which expresses the matter in strict scientific terms. While some of these terms are incomprehensible literally, like infinity, they are useful working symbols—but not "facts." In this sense, perhaps, Dr. J. E. Wishart[5]

[5] Hibbert Jour., 38: 447, July, 1940. Dr. Wishart, a distinguished theologian, in kindly reviewing the manuscript of my essay, roundly condemns my assumptions and conclusions and gently expresses a fitting pity for me and mine. More seriously he doubts that there can be religion without God, and thus feels that my working definition of religion is unacceptable. The Right Reverend Edward Lamb Parsons, Bishop of California, also was generous as ever to be interested in my manuscript, and he has somewhat the same opinion as Dr. Wishart. Both feel that attempted coercion by arbitrary authority may

proposes that we pragmatically use the idea of a first cause, incomprehensible though it may be, because it explains the situation better than any other hypothesis on the problem.

### Concluding Notes

It is natural for the Reverend Professor O'Conor, being a physicist and seismologist, to develop his position from the standpoint of physics and mathematics. But in requesting consideration for a scientific approach to religion he can not neglect other fields of scientific endeavor, particularly biology, since man is so closely and clearly related to other mammals, albeit more remotely to other living things. One may wonder whether there was deliberate omission of any consideration of the possible bearing on religion, faith or belief, of current contributions in physiology, psychology and psychiatry. To one familiar with recent advances of knowledge in these fields, the anthropomorphic features of religion are not accidental. If the explanations of man's behavior in relation to his environment as offered by scientists in these fields, are considered to be scientifically valid, then the premises and position postulated by the Reverend Professor O'Conor and by conventional religious dogmas of all sorts, become dubious indeed.

Since the Reverend Professor O'Conor refers to miracles, we might pay our respects to them here in passing. Without even raising the question of the degree of probable accuracy of the canonical accounts, one may apply our knowledge of psychological and psychiatrical processes to the miracles as reported, and find that they are capable of receiving full and adequate natural explanations. Since my statement is a contradictory opposite of the Reverend Professor O'Conor's, one's judgment in the matter depends on estimating the degree of probability of correctness of correlation; but with what standard? An attempt has been made to define the standards of science and religion. Which do you choose? For a scientific approach to religion, one would expect that scientific standards would be applied to religious claims.

We order our lives on the basis of what we believe. The source and character of our beliefs appear thus to have great importance. If our faith or religion consists of expressions of fond hopes, pathetic desires and anxious wishes, inspired by greed, jealousy and fear, nothing very satisfactory for ourselves or society is likely to result. On the other hand if our faith or religion is based, not upon what we may want to believe, but upon what is possible for us to believe in view of the limitations imposed on us by our knowledge of ourselves and our environment, it would seem that a little more reasonable and individually responsible ethic might be derived. This is Warner Fite's "Individualism," New York, 1911, in which he points out that the interests of conscious individuals are harmonious. It is the point of Dewey's suggestion of harmonizing desire with knowledge or experience, and of what Walter Lippmann thinks was meant by Confucius, Jesus and Buddha.

Professors E. G. Conklin and C. J. Herrick have come independently to this conclusion from scientific considerations relating to biology.[6] With them Professor S. J. Holmes[7] and Dr. George Sar-

be as dangerous in religion as in science. Father O'Conor graciously acknowledged receipt of the manuscript, and commented generally on it, but has not yet had time to dissect it in detail. The italicized statement may be made more general as follows: *The reality of any moment is as asymptotic to a first and unproduced cause or to a final and end result as it is to infinity in any direction of space or in time either past or future.*

[6] Scientific Monthly, 49: 99–110, 295–303.
[7] *Science*, 90: 117–123.

ton[8] also independently agree, that, to put it simply, the probability of survival of human relationships increases with the degree of mutual adjustment, in the relationship, toward mutual satisfaction.

Although stated in the form of a scientific generality, the ethical significance of this principle appears in relation to the common urge for survival and satisfaction. Consciousness of the operation of this principle suggests the wisdom of such altruistic, considerate and magnanimous conduct as is intuitively considered "good" in all ethical systems. The social customs and conventions now with us have so far exhibited survival value in the Darwinian sense. We may apply evolutionary principles to them, and attempt the formulation of a *modus operandi*. Such a formulation constitutes the statement offered. Whether or not it may give a biological and scientific basis for a pragmatic ethic remains to be estimated.

A scientific approach to religion implies the problem of whether or not science can form a basis for religion. The answer is "yes" in the light of the definitions proposed by me, but not on the basis of those proposed by the Reverend Professor O'Conor. This conclusion, not being satisfying to him, will probably be rejected by him, for man remains the measure of all things.

To one impatient with the problem of philosophy in coordinating knowledge and desire, this discussion may appear to be circular. Indeed, unless the Reverend Professor O'Conor and folks like me can agree on definitions of religion and science, we may always find ourselves in the position Fitzgerald ascribed to Omar of coming out the same door wherein we went.

[8] "The History of Science and New Humanism," Harvard, 1937.

# SCIENCE AND TRUE RELIGION

## A REPLY TO DR. C. D. LEAKE

By The Reverend JOHN S. O'CONOR, S.J.
PROFESSOR OF PHYSICS, GEORGETOWN UNIVERSITY

At the risk of ensnaring further unwary scientists and unsuspecting laymen I am taking this opportunity to point out in some detail the perhaps unconscious naiveté of Dr. Leake's assumption that his criticism of my article "A Scientific Approach to Religion" constitutes a "logical answer" to the same.

His reply is, however, most appropriate at least in one respect; for it serves as a perfect example of that procedure to which I specifically called attention: namely, that of holding up to ridicule some synthetic religion, made to order for the purpose from elements which constitute but a catalogue of inconsistencies and excesses, and by this means attempting to discredit true religion, which has no more to do with this caricature than astrology and numerology have to do with true science.

Dr. Leake begins by taking exception to my definition of religion and substitutes one of his own making which is: "A faith or belief one has with respect to the relation between one's self and one's environment."

I have consulted Sir James Murray's ten-volume New English Dictionary, including the 1933 supplement, and find neither philological nor historical warrant for such a definition. Neither Webster's New International nor Funk and Wagnalls' New Standard dictionaries gives a definition which can be considered the approximate equivalent

of it. Thus the truth of the statement that "according to the dictionaries, current usage permits this broader definition" is yet to be established.

The definition which I submitted *is* solidly established, both historically and philologically, and is the one generally agreed to—conventional, to use Dr. Leake's own characterization.

To take such a definition, and then show by a reasoned proof that the essential element in it (A Supreme Being) has objective existence, or to make up a definition for the occasion, which on analysis displays no criterion for differentiating between the thing defined and the multitude of other reactions to environment which all of us know are *not* religion, which of these two procedures is the more scientific and objective; which, I ask, is the more arbitrary and postulational? When Dr. Leake counters with the statement that because religion historically usually includes the recognition of a Supreme Being—this is no reason why it always must—he is arbitrarily demanding a change in the significance of words. Is it not more in the spirit of science to discuss *what is and what has been* rather than *what might be?*

Dr. Leake cites my neglect to define science, yet later on disagrees with what he calls my definition of the scientific method. He claims that science is more concerned with the establishment of facts than with the attempt to fit them into a system or theory. I question that distinction. Albert Einstein is quoted as defining science as "the coordination of our experiences by bringing them into a logical system." Perhaps he has been misquoted, perhaps his definition is not correct, but I doubt very much that Dr. Einstein will cease working on a unitary field *theory* for the facts of gravitation and electromagnetism when he becomes aware that this is not an essential aspect of science. I also doubt that the successors of Darwin will all repudiate their work as unscientific because they have been attempting to fit the facts of paleontology into the theory of evolution.

An adequate discussion of Dr. Leake's section entitled "Aim, Spirit and Method of Science and Religion" is precluded by the scientific nature of the journal in which this discussion appears, for the entire sequence is made up of paragraphs which are introduced by such subjective indicators as "we may assume," "I believe," "it seems," which are used sixteen times in presenting what purport to be acceptable views on religion, while the attacks on my approach are shot through with a dogmatic positivism which can best be met by a counter-demand for proof.

Near the end of the section just referred to, as well as in subsequent parts of his article, Dr. Leake claims that my "approach" exemplifies the usual method of religion, which is to "assume God and immortality and then relate everything else in experience to these assumptions." Again where an answer is attempted to my so-called "leading questions" the following is found: "I think it is clear . . . that dogmatic religion is incompatible with the scientific method as long as dogmatic religion *requires the postulation* of a first and unproduced cause of the universe or of the *necessary assumption* of any other idea for which direct objective evidence is lacking."

First, let me make it clear that immortality was not even mentioned in my entire article. Secondly, let me emphasize that it is a *proof* of God's existence and not a *postulate* that was the object of my investigation, not, it is obvious, with the purpose of making an exhaustive study of the same, but merely to call to the attention of readers of THE SCIENTFIC MONTHLY the similarity between the methods of the so-called "time-worn" arguments and those of

modern science. In confirmation of this I again call the reader's attention to the reference given near the end of page 369 in my article. This book (Brosnan's "God and Reason") is a standard text on natural theology in which the proof under discussion is fully developed.

To say that the "proof consists chiefly of begging the question and of vague analogy" is merely to confirm the suspicion that my critic has never taken the trouble to look up the reference in question. If the Doctor had disagreed with the proof referred to and pointed out its alleged fallacies, a basis for further discussion might be had, but to call a proof a postulate seems inexplicable even in the case of one for whom the realm of philosophy is but a "wilderness."

In the discussion on "Authority" much is made of the objectivity of science as indicated by the exclusion of "wishful or fearful thinking" and of the refusal to admit "either bias or commitment *a priori*."

As a scientist I concur completely on these points, but should like to call attention to the third work listed in Dr. Leake's introduction as an "outstanding source," *viz.*, A. D. White's "History of the War of Science with Theology in Christendom." It is amusing to listen to talk of "discounting judgments which may be influenced by psychological . . . or emotional factors . . ." when the author of a book, which is filled with such violent diatribes that it can not be considered by any impartial critic except as a piece of rank bigotry, is held up as an outstanding authority. The inhibitions, attributed by Dr. Leake to most of his colleagues, which prevent scientists from writing on topics they fear will give offense to those interested in religion, would undoubtedly become overwhelming should they consult this reference. But no scholar will be tempted to accept the writings of A. D. White as source material.

My plea for deference to "authority" is characterized as one which "has serious implications these days." It is hardly necessary to point out that the word "authority" should have been understood as meaning *credibility*, namely, that which gives weight to one's testimony, *i.e.*, knowledge and honesty. The politico-social sense of the word authority is quite irrelevant here. To invoke a dubious historical argument against the authority of the Church, based on an ambiguity in terminology, and to reject the historical argument in relation to the notion of God in religion does not seem to be consistent.

I am sure that Dr. Leake would resent, as would any other scientist, were one to claim that it is "science" which is devastating the earth and sweeping democracy from its surface. Yet all will have to agree that science and its principles are helpless to prevent the devastation wrought by those who admit no authority (in the sense taken by Dr. Leake) higher than their own.

There is one statement of Dr. Leake's with which I am at least in partial accord. He says: "Granting Father O'Conor's assumption of the objective existence of God, the rest of his position follows logically. . . ."

A change of the word *assumption* to *proof* would bring apparent agreement on one point.

But even if the word proof were substituted the difficulty would merely be pushed further back and the question could legitimately be asked: Are there not postulates in the proof?

To get at the root difference between Dr. Leake's position and mine we must also ask: Is any scientific conclusion which is beyond the direct evidence of the senses susceptible to "proof"? My answer is yes. There are certain analytical propositions (such as the principle of contradiction and of sufficient reason) which are objectively and immediately evident.

These are not mere postulates but

necessary laws of thought which flow from the laws of being and without which there could be no knowledge. These principles when properly employed in conjunction with sense data lead to certain necessary conclusions regarding the reality of the external world and its origin. Thus if one admits objective evidence and the supremacy of reason to the extent of accepting conclusions deduced by it, although these may go beyond immediate sense data, then one must also admit the absolute validity of the cosmological proof for the existence of God.

If one denies these principles of metaphysics and poses as admitting only knowledge derived directly from sense data, then one is forced to admit that there is no difference between scientific knowledge and the lowest form of animal reaction to sense stimuli. This position of pure sensism, or of positivism, is itself a dogma which has never been rationally established and is rooted in a prejudice which refuses to accept the implications of reasoning.

To preclude the necessity of maintaining an entirely absurd position the laws of thought must at least be assumed as a postulate system—otherwise our sense data could not even be communicated rationally, much less discussed or correlated scientifically.

While not agreeing with or admitting a system of philosophy which claims that all fundamental principles are but postulates, we do maintain that, as far as this controversy is concerned, any postulate system which admits the possibility of scientific knowledge, as it is commonly understood to-day, is also sufficient to establish the existence of God on the same scientific basis. Science to-day is concerned with, reasons about and concludes to propositions which are certainly beyond the direct range of sense data. If this is admitted, the arguments for the existence of God *a fortiori* can not be thrown out unless one is willing to exclude all such concepts as electron, proton, neutron, relativity, quantum theory, and the like.

Let it be clearly understood that the position presented above is not put forward as the alpha and omega of religious systems. It can be considered, however, as the rational basis of natural religion and serves as a preamble to supernatural faith, which is a gift of God, reasonable in itself but not obtained by reason alone.

Regarding the bearing on religion of current contributions in physiology, psychology (experimental) and psychiatry, my reply is that the omission of such considerations by me was indeed deliberate, not through any fear of consequences to true religion (for truth is one and can not contradict itself), but on the principle that it is scarcely profitable to discuss such disciplines unless one's knowledge approaches that of the professional both in depth and interest. Any reply may well be deferred until something more than the hypothetical proposition advanced by Dr. Leake is forthcoming.

The categorical denial of my remarks on miracles has in no way advanced matters on this point. Before making any choice, the prospective student of miracles must become acquainted with the literature. I am therefore appending a few references which may serve as an easy introduction to the subject and will have to be examined and evaluated before there is any serious attempt to posit a judgment.

To close on a note of unity let me express entire accord with the idea that neither my own five-page article, nor Dr. Leake's one of more than twice that length, can be expected to do more than stimulate further interest in a subject that has been vital to men of all times.

If this has been accomplished we can relinquish the discussion with sentiments

akin to those of St. Augustine, and quote from his "City of God," as appositely we hope, as Dr. Compton did last January from his "Commentary on Genesis."

If we should bind ourselves to give answer to every contradiction that the opposing front offers (how falsely, they care not, as long as the denial is made), you see what trouble it would be, how endless and how fruitless. Therefore . . . I would not have you read this volume thinking I am bound to answer whatsoever you or others shall hear objected against it, lest you become like the women of whom the Apostle speaketh, that they were "always learning but never able to come to the knowledge of the truth."

## Summary of the Cosmological Argument

*Proposition to be proved:*

There exists an unproduced first cause of all things existing in the world.

*Proof:*

There exist in the visible world produced beings.

But the existence of even one produced being necessarily implies the existence of an unproduced first cause.

Therefore such a cause exists.

The major proposition of the proof is evident from internal and external evidence.

The minor proposition may be proved as follows:

The cause of the produced being in question is either unproduced or produced.

*If unproduced our conclusion stands.*

If produced the question recurs: Whence its producer?

The answer must either finally stop at a *first unproduced cause* and again *our conclusion stands*, or,—

We must admit an infinite series of successively produced causes either dependent on an unproduced cause, or without such a cause. If the series depends for its existence on an unproduced cause *our conclusion again stands.*

If however it is asserted that such a series can exist without an unproduced cause, to *finally* establish our conclusion we must prove this assertion false.

In other words we must prove that—

An infinite series of successively produced causes, without an unproduced cause of it, is absolutely impossible.

It *is* impossible if nowhere can be found an *adequately sufficient* reason for the existence of any one member of it. But nowhere in it can such a reason be found.

It can not be found in the being itself, which we shall call $A$, for $A$, a produced being, could then never have existed, for a being can not produce itself, before it itself exists.

Nor can it be found in any prior cause. For if the *adequately sufficient reason* for $A$'s existence could be found in any one such cause or group of causes then all causes in the series prior to this group could be considered as non-existing as far as their requirement for $A$'s existence is concerned.

But if any cause in the series is considered as non-existing then all subsequent causes in the series must be considered as non-existing and hence $A$ as non-existing.

Hence the hypothesis, that any cause in the series can give an adequately sufficient reason for $A$'s existence, results in the absurd conclusion that $A$ can not be existing.

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# BOOKS ON SCIENCE FOR LAYMEN

## THE CURRENT WHALE OIL INDUSTRY[1]

THE aim of this book, as stated by the author, is "to analyze the economic problems surrounding one of the world's important raw materials of the present time." Whaling, which contributed so much to the wealth of Dutch, Hanseatic, British and American citizens, is traced from its beginnings, the causes of its decline are reviewed, and the background for its revival under Norwegian leadership during the twentieth century is described.

The book commences with a discussion of the importance of whale oil as a commodity in world trade, where it constitutes about 9.4 per cent. of the total volume of foreign traffic in fats. Germany, for instance, in 1935 obtained 54 per cent. of her margarine and lard compounds from whale oil. Whale oil contributed 41 per cent. of the margarine, 28 per cent. of the lard compound and 16 per cent. of the soap manufactured in Great Britain in 1937.

Although whale oil is the chief product of the modern whaling industry, the commercial utilization of a whale carcass yields also whalebone, ambergris, meat, concentrated protein foodstuffs for domesticated animals, fertilizer, hormones, leather and glue. Hence "the value of whales as a natural resource depends essentially on the demand for certain raw materials to be derived from it, on man's technical ability to utilize it, and on his intelligence in refraining from depleting it."

Whaling is now prosecuted largely on the high seas beyond the jurisdiction of sovereign states. It is of especial importance to countries which lack natural resources but have an abundance of labor and industrial facilities to build floating factory ships, whale-catcher boats and transport vessels, and to utilize commercially the products obtained. Consequently, intense international competition has developed into a struggle between the British-Norwegian interests which view whaling as a means for the employment of manpower and capital and the German-Japanese companies which are fostered by their governments to obtain fats and raw materials either for domestic consumption or for conversion into necessary foreign exchange.

Despite efforts at international regulation sponsored by the League of Nations in 1931 and by the Norwegian and British Governments in 1937 and 1938, it has become increasinly apparent that the high standard of efficiency attained by factory ships and whale-catching equipment threatens the continuation of the industry. One obvious shortcoming of these agreements was the reluctance of most of the countries to impose a definite limitation of oil production by fixing maximum quotas for their own whaling industry. In the international exploitation of this resource the competing nations are endeavoring to obtain a maximum share in the catch and at the same time are voicing their fear of excessive slaughter of existing stocks of whales.

The author believes that whales will be amply utilized so long as competing nations find whaling a remunerative enterprise and perhaps even longer by companies subsidized by countries with a deficiency in fats and foreign exchange. World production of whale oil each year is determined by the number of whales caught, by the ratio of blue whales to other kinds of whales in the catch, by the average size of individuals of each species in the catch and by the technical efficiency and economy of oil-processing operations.

[1] *Whale Oil, An Economic Analysis.* By Karl Brandt. Illustrated. xiv + 264 pp. $3.00. June, 1940. Food Research Institute, Stanford University Press.

Since the price of whale oil is mainly determined by prices of competitive fats and since the costs of whaling increase with the decline in the average size of whales caught, these considerations control the remunerative operation of factory ships equipped with costly processing machinery and of the whale-catcher boats that do the killing. New uses of whale oil as food and industrial material and improvement in quality by prompt processing of whales caught has brought about an expanded and diversified demand for this fat. The author elucidates the highly complex play of factors affecting the prices of oil and fats and concludes that whale oil has become the base of the price structure of fats.

The discussion of the transport, storage and marketing of whale oil is followed by a résumé of the influence of tariff duties, excise taxes and other measures devised to protect domestic producers of competitive fats on the marketing of this commodity. The appendix is designed primarily as a source for selected statistical data.

In this well-documented and carefully prepared book, a qualified, impartial observer makes a notable contribution toward a better understanding of the mechanics of the whaling industry.

REMINGTON KELLOGG

## FROM BACTERIA TO ORCHIDS[1]

PROFESSOR HYLANDER has written a book such as every botanist has wished he could write but which none of us has been able to produce. It is a big book, over 700 pages, $7\frac{1}{2}$ x $9\frac{1}{2}$ inches, crowded full of remarkable photographs and meaty text in fine print.

This is probably the most comprehensive book on plants that has yet been written for the general reader.

Mr. Hylander classifies and discusses virtually every common type of plant now extant in this country—native and naturalized; he tells of their distribution, their habits, their uses and their various unique and specialized structures which enable them to exist in specific environments.

Here is a world of detailed and exciting information on plants—all the way from bacteria to orchids. The author writes of the seaweeds and shy organisms that grow on the ocean floor; the plants that form a felty mass on the sides of cliffs; the vegetation that stands primly erect in a quiet marsh or lies asprawl on the stagnant surface; the familiar flowers and trees of our American fields, forests and deserts. Here one can learn about plants that kill insects for food; plants that live in cooperative colonies, dividing their labors with evident success; and scores of others. So inclusive is this book that it will be appreciated by amateurs, students and experienced botanists alike.

In gathering his material, Mr. Hylander, who is assistant professor of botany at Colgate University, traveled twice from Maine to California by car and trailer; he has included here over 400 of his plant photographs and line drawings. The book is as fascinating as the world of plants which it describes.

Probably the most serious fault in the book is that it is too big and the print too small. The type would not have been too small if the page had been broken into two columns, but, set as it is in long lines stretching across the wide page, it soon puts the reader's eyes aswim and he lays it down with a sigh only to be intrigued to pick it up again because of its interest. This defect is mitigated by the fact that the book will be used more for reference than for continuous reading.

In a book so large one could readily pick up errors and find many details to criticize. The very numerous line drawings are nowhere near as skilfully done as the photographs and often leave off the details a botanist would wish to see (and which could have been added with

[1] *The World of Plant Life.* By C. J. Hylander. Illustrated. xxii + 722 pp. $7.50. Macmillan Company.

just a little more effort). Too many of the halftones have lost the "snap" evidently possessed by the original photographs and many of them are smutted against the next page by careless printing. There are some expressions that are rather forced in an effort to popularize, like "warfare" among plants which refers chiefly not to competition in their struggle for existence but to parasitism.

But one is ready to forgive the shortcomings of the book for the service it does him. It is to be hoped that a second edition will soon be required and give the author opportunity to make all desired improvements.

ROBERT F. GRIGGS

## TWO QUAKER BOTANISTS, FATHER AND SON[1]

THIS, the second volume of a series, "Pennsylvania Lives," gives a very brief account of the lives of two Quakers who had a profound influence on the colonies and became valued friends of their European correspondents. John, the father, with a meager school education became so deeply interested, first in plants and later in many fields of science, that he succeeded in training himself to become a valuable contributor to scientific knowledge. He traveled thousands of miles in the eastern United States from Canada to Florida in his collections of plants and seeds, many of which he sent to European botanists, to whom they were unknown. His descriptions of plants, weather, animals and soils were both accurate and thorough.

John Bartram brought to his farm near Philadelphia parts of his plant collections which with his European exchanges became the first American botanical garden. In this garden he became one of the first to understand sex in plants and to cross-pollinate them.

William Bartram, the son, began his travels with his father at the age of fourteen, on a trip to the Catskill Mountains. William learned to love nature and travel, but he appears to have been so interested in philosophy that his collections and exchanges with his correspondents suffered. He inherited his father's power of keen observation and his descriptions were famous for their vividness. Many of them appear to have been the bases for later descriptions by famous authors. The "Travels," a masterpiece, was published in many parts of the world.

Since these men set so high a standard for accomplishment as to draw the eyes of the world toward Philadelphia, it is to be regretted that so few quotations could be included in this small volume and that the number of copies of the edition is limited to 1,000. It would seem that the reading of this volume might well be a stimulus to many others to overcome difficulties and make contributions to their chosen fields.

L. EDWIN YOCUM

[1] *John and William Bartram.* By Ernest Earnest. Illustrated. xvi + 187 pp. $2.00. 1940. University of Pennsylvania Press.

## A PRIMER OF ANTHROPOLOGY[1]

A SUCCINCT popular treatise on man's origin and on early man, by "formerly senior member, scientific staff, Anthropological Department, Welcome Museum," London. Deals with The Beginning of Things; The Ancestors of Man; The Great Ice Age; Men of the Ice Age; The End of the Ice Age; The Men Who Came after the Ice Age; The End of the Old Stone Age; The Middle Stone Age, and The New Stone Age.

A very readable production that may, in another edition, make a good primer on the subject it deals with; but before that it will need considerable mending in various details. Thus the meteorites (p. 7) are hardly "fragments of metal which probably made the original body of the sun"; the skulls of all the anthropoid apes are not all "much rounder"

[1] *Mankind in the Making.* By M. C. Borer. Illustrated. 152 pp. $1.50. 1939. Fred. Warne and Company.

than that of the Pithecanthropus; it is not true (p. 41) that "no other relic of any other member" of the Pithecanthropus family has ever been found; the "migrations" of early man from Africa or Asia to Europe, the separateness, chronology and extinction of the Neanderthal man, and the coming in from some unknown somewhere of the full-fledged *Homo sapiens*, are all outlived assumptions. And there are inaccuracies about the Eskimo and in other matters.

Yet the treatise can not be condemned and may in a future edition be made quite useful. There is a great man in England, Sir Arthur Keith, who for the sake of soundness would probably be glad to assist in setting things straight.

A. H.

## DEVELOPMENT OF THE HUMAN EMBRYO[1]

It is a pleasure to welcome the fourth edition of Arey's "Developmental Anatomy." To those who have used this volume on human embryology it needs no introduction. It is still the outstanding text-book in its field.

Although the book is written primarily for the use of serious students, it should not be overlooked as an interesting source book by those who at one time or another become interested in the phenomena of prenatal development. No one can pass by the miracle of reproduction without hesitating to consider the underlying organization and motivation that produces a complicated functional organism from the union of a sperm with an egg. There is a real fascination in the history of development through segmentation, formation of layers of cells and the eventual foldings and growth processes that produce the various organs of the foetus.

There are a great many readers who, having perused a lighter, more popular book on human embryology, will want to learn in greater detail or with closer accuracy what is happening as pregnancy proceeds and the baby develops. This volume will answer those questions in an authoritative reliable fashion as it does for the medical students into whose hands this book will find its way. It is well illustrated, carefully written and an entirely worthy volume.

Ira B. Hansen

[1] *Developmental Anatomy.* By L. B. Arey. Illustrated. xix + 612 pp. $6.75. 1940. W. B. Saunders Company.

## "LIVING WAVE FUNCTIONS"

In Dr. Gamow's review of my book, "The Soul of the Universe," in the December number of Scientific Monthly he writes that I have introduced the notion of "Immaterial Living Wave Functions," and then proceeds to show that wave functions have no physical counterpart in the external world. I have never in my book used the term "wave function," and obviously not the nonsensical term "living wave-functions." Instead I have used the terms fields of force, space-time structures, frequency patterns and wave systems as descriptive of *directly observable* space variations and time fluctuations of physical characteristics. If the variations at a particular place or places are rapid and fluctuating they can often be described in terms of waves or frequencies. For instance, the fluctuating electric field around many neurones in the brain can be described as "wave systems," a description which is entirely independent of any ideas about phase waves or wave functions. In describing the phenomena of life we must deal with the directly observable phenomena in space and time, without introducing any mathematical representation in multidimensional configuration space for which we obviously are not yet ready. Gamow's objection loses its force when we realize that we are dealing with structures in space and time which are, at least in principle, observable by our sense organs aided by suitable instruments.

Gustaf Stromberg

DR. IRVING LANGMUIR

# THE PROGRESS OF SCIENCE

## DR. IRVING LANGMUIR, NEWLY ELECTED PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

IRVING LANGMUIR was born in Brooklyn, New York, in 1881. His early school days were spent in Paris, France, and at the Chestnut Hill Academy, Philadelphia, Pennsylvania. After a course at Pratt Institute High School, he studied at the Columbia School of Mines, graduating in 1903 as a metallurgical engineer, and then studied physical chemistry with Nernst at Goettingen, Germany. Subsequently, he taught chemistry at Stevens Institute for three years. He joined the staff of the General Electric Research Laboratory in 1909, and since then has been continually active as a research scientist. During the past eight years he has been associate director.

He has been honored by degrees from many universities, including Oxford, Johns Hopkins, Harvard, Columbia and Princeton. For distinguished scientific services he has been awarded fifteen medals and other tokens. Among these are the Nobel Prize of the Swedish Academy of Science, in 1932, the John Scott Award of Philadelphia, and the Hughes, Perkin, Nichols, Chandler and William Gibbs medals.

His scientific contributions are best described in the 217 scientific papers which he has published. Because of the nature and number of those widely distributed contributions I can not review them adequately here. In choosing one line of his thoughts I might select a purely chemical one, but I should soon be at the end of ordinary vocabularies. Or I might take some attractive physical, electrical or illumination problem, but within each of those confines I should still feel that I had failed to picture him properly.

Integrating his work, I see that the scientific advances are made just at the extreme, almost invisible ends of paths of human interest, where part of the job consists of laying down hard new words as a sort of corduroy road.

As a start along one road you may enter the realm of Langmuir's radio tube contributions through an article by President Karl Compton, of the Massachusetts Institute of Technology, in a current issue of *Science*. But there were years of frontier-research involved, even there. Langmuir had to distinguish quantitatively the specific and different electrothermic emissions and study the migration rate of thoria, for example, in solid, heated tungsten, together with the surface films of chemically reduced thorium, before the countless types of radio tubes could be evolved. In addition, there were all sorts of studies of the electrons as they wandered about in vacua, influenced by different electrical forces. When mere traces of gases were present, entirely new phenomena were presented. Such words as "space charge," "bombardment," "positive ions" and "charged grid," brought out still more new names, such as pliotron, kenotron, thyratron, etc. Laid down at the right time and place, they have "made going easier" in many new territories.

While it seems to me that all his work is strongly tied together by his simple conceptions, it is still impossible for me to illustrate it. If I try to explain his contributions to "molecular films" I can not be clear enough, though he has paved the way in many American scientific circles by showing his fundamental monolayers. While these foreshadow infinite chemical research leading into processes of life and growth, living cell permeability, etc., the implications of such work may not be speculated upon here. He has directed his efforts so far as to deter-

mine form and structure, in parts or whole, or many sorts of organic molecules. This has included the immensely complex insulin. But I may confuse by even mentioning this.

Perhaps it is better to say that, just as Langmuir has contributed greatly to our elementary concepts of chemical valency and made us almost see the various kinds of bonds which express interatomic attractions and molecular coherence in such simple cases as helium and atomic hydrogen, and the "attractions" between hydrogen and chlorine in hydrochloric acid, so also in working with simple soap films he has opened up new conceptions of existing matter, such as two-dimensioned solids, liquids and even gases, together with their interreactions. And this he has done by simple means but with quantitative results.

Langmuir seems to love the simple—the simple, inquisitive youth, the simple, direct question and answer. His well-trained, questioning attitude makes him a fine, forward-seeing teacher of old and young. His physical energy and mental virility are very unusual. By his efforts he makes the subject of his interest important. Repeatedly investigating an apparently insignificant scientific point, he makes it most fundamentally significant. At the same time, as the resulting chips fly, he watches the direction of prevailing winds of utility or service.

Take, for example, that highly useful discovery, the gas-filled incandescent lamp. We see it every night, but how many understand it? How can the same size of glass bulb, with the same element, tungsten, have three times as much light-efficiency when, in place of a vacuum, it is filled with gas? Gas is obviously a greater heat dissipater than the vacuum, and the vacuum offers no obstruction to the light-emission of the filament. We had apparently almost exhausted all practical possibilities of making better vacua in our search for lamp improvements, but Langmuir first further improved even the methods. Then he carefully studied every imaginable thing left. What was happening to, with, through, by and/or among the adherent traces of persistent gas-residues? He experimented. After years of this, he learned the remarkably different effects of water vapor, nitrogen, oxygen and hydrogen where these elements were almost nonexistent. Space limits explanation here, but he learned about atomic or dissociated hydrogen, decomposed water, blackened and browned bulbs, rate of simple evaporation of the filament, and that each was dependent upon the others. Then he visualized the new advantage to be gained by coiling tungsten filaments in certain sized helices (instead of using simple "hairpins") and operating them in definite pressures of nonreacting gases. And so the new and better lamp.

The long lines of reasoning by simple, proved paths, but just beyond the former terminals, are to be seen in all his scientific work.

Willis R. Whitney

## NOTABLE SCIENTIFIC PROGRAMS AT PHILADELPHIA

Probably no other scientific meeting ever presented so great a variety of notable scientific programs as that of the American Association for the Advancement of Science at Philadelphia from December 27, 1940, to January 2, 1941, inclusive. In those six days 222 scientific sessions were held before which 2,164 addresses and papers were delivered or read.

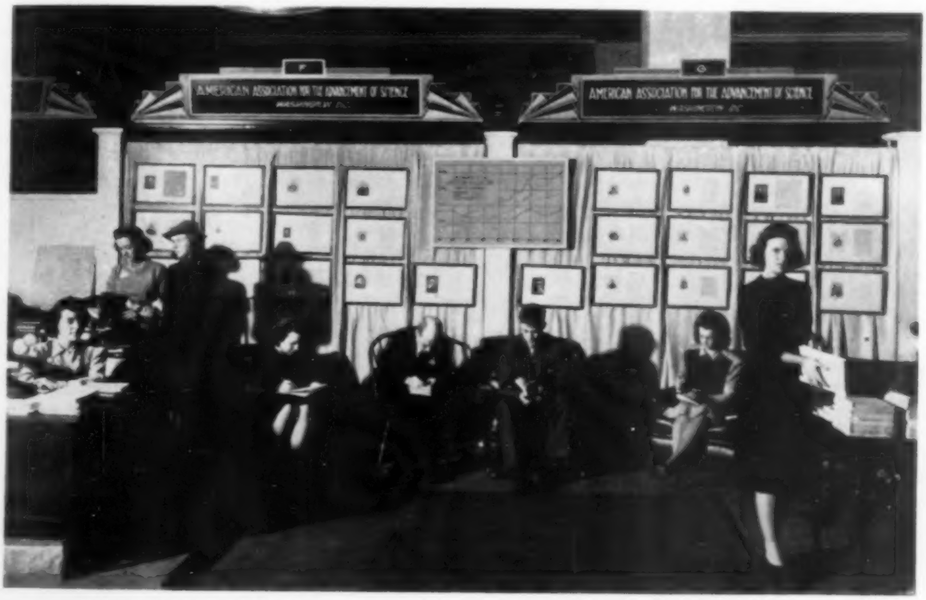
Instead of attempting the impossible task of referring to all the programs of the meeting, a few will be enumerated, each of which was a joint discussion of some important field of science by a number of eminent specialists. The first on the list in the number of participants and perhaps in the importance of the subject was the symposium on "Alcoholism." There were 47 contributors to

this discussion which, in six half-day sessions and one evening session, considered the subject in (1) its physiological and chemical aspects, (2) its clinical aspects, (3) its neuropsychiatric features, (4) its treatment and prevention, (5) its social and legal problems (two sessions) and (6) a general session presided over by Dr. Thomas Parran, surgeon-general of the U. S. Public Health Service. The participants in this notable program were leading specialists in the problem of alcoholism from all sections of the United States. It is expected that this comprehensive and correlated group of papers will be published in one volume, probably by the American Association for the Advancement of Science.

Another program of similar scope and completeness was the symposium on "Human Malaria" in which there were 42 participants, including the foremost specialists in the field in this country. The malaria problem is of great importance in the southern states and the insular possessions. It is especially important at present because of the large number of army camps that are being established in the South in the national defense program and because of the large number of men who will be employed in constructing naval bases in the Caribbean and West Indies regions.

Not all the large programs, however, were on medical subjects. Three of an entirely different character were "A Scientific Basis for Ethics," "Science and Value" and "The Scientist and American Democracy." Since all three of these symposia are similar in that they are concerned with the significance of science in moral and social questions it has been suggested that the association publish them together in one volume.

Another distinguished program of a quite different type was organized in




AMERICAN ASSOCIATION BOOTHS AT THE SCIENCE EXHIBITION

TWO OF THE EIGHTY OR MORE EXHIBITS HOUSED IN CONVENTION HALL, SHOWING IN THE BACKGROUND A SERIES OF PORTRAITS AND LETTERS FROM PAST PRESIDENTS OF THE AMERICAN ASSOCIATION. THE PICTURE OF THE FIRST PRESIDENT, W. C. REDFIELD, CAN BE SEEN IN THE UPPER LEFT-HAND CORNER. MR. SAM WOODLEY, THE ASSISTANT SECRETARY, HAS COLLECTED A SET OF THESE PORTRAITS AND LETTERS—ONE EACH YEAR SINCE THE FOUNDING OF THE ASSOCIATION 92 YEARS AGO.

celebration of the centenary of the publication of one of the most influential books in the history of chemistry, namely, Liebig's "Organic Chemistry in the Applications to Agriculture and Physiology," the first edition of which appeared in 1840. The participants in this symposium discussed, with adequate references to the literature, the amazing and often quite unexpected developments of the regions which Liebig entered a century ago.

To mention a program at the opposite extreme of human interest, the American Philosophical Association presented a symposium on "The Problem of Religious Knowledge." And again, the geologists looked into the earth in their two sessions on "The Igneous Rocks of the Appalachian Mountains System," while the astronomers looked up to the stars in theirs on "Intrinsic Stellar Variation."

The few programs that have been mentioned illustrate the enormous diversity of the aspects of science. They are as varied as the interests of the human mind; indeed, they are more varied, for science is now involved in every human activity and is rapidly creating innumerable new interests—and problems. The rapid changes that are taking place in the world as a consequence of science are disturbing to many persons, especially to those gentle souls who find happiness in drifting with the tide. But reason and the whole history of the earth, and of the

THE SYMPHONY ORCHESTRA ABOUT TO PLAY FOR THE ASSOCIATION
SHOWING ONLY ABOUT HALF OF THE SWARTHMORE GROUP; DR. SWANN IS STANDING.

Naturally the world war stimulated the organization of certain programs, the most obvious being "Psychology and the National Emergency." One of the participants was Dr. Leonard Carmichael, president of Tufts College and chairman of the committee which is preparing, for national defense, an essentially complete roster of the tens of thousands of American scientists, together with their respective preparations, qualifications and preferences for service.

life on its surface, teach us that improvements come only with changes and that dinosaurian complacency with existing conditions leads only to stagnation and extinction. Those who participated in the programs that have been enumerated, and in many others, prefer to pursue the entrancing and adventurous paths of science.

The scientist, however, is not simply a rigid experimenter or a cold logical machine. Whatever rare qualities he may have while he is working in his own special field, he is nevertheless a human being and shares in the weakness of human beings. He also shares with artists, often to a very exceptional degree, an appreciation of and skill in various arts. Mathematicians and astronomers have long been noted for their taste for music. Of course not every mathematician has musical talent. Possibly the percentage of mathematicians who love music is not greater than that among other persons of similar culture. The tradition may have arisen because it seems incongruous to a non-mathematician that a mathematician should be interested in artistic things. There appears to be no claim that musicians as a class have exceptional mathematical ability.

Whatever the statistical facts may be, it is highly probable that scientists are at least as gifted in art as the average person. They must be, for science at its highest level is art. It produces esthetic effects similar to those produced by music, poetry and painting. The recently elected president of the association, Dr. Irving Langmuir, is an artist in everything he does. He listens to symphonies musicians never heard and sees rainbows that never appeared in the sky. In his work he builds beautiful structures that the language of ordinary mortals can not describe.

All I have said about scientists and music was illustrated at a tea for the

DR. SWANN PLAYING THE CELLO

EXECUTING MUSICAL ENTERTAINMENT
FROM LEFT TO RIGHT: J. STOGDELL STOKES, CHAIRMAN OF THE ENTERTAINMENT COMMITTEE FOR THE ASSOCIATION; DR. W. F. G. SWANN, DIRECTOR OF THE SWARTHMORE SYMPHONY ORCHESTRA, AND LUCIUS COLE, CONCERT MASTER.

members of the association provided by the local committee in Philadelphia. A great physicist, mathematician and scientist led the Swarthmore Symphony Orchestra of about seventy members in rendering a splendid program of classical music. This remarkable leader was W. F. G. Swann, director of the Bartol Research Laboratory. Concerning the composition of his orchestra, which was founded in 1936, he says:

"Our membership covers a very wide field of professional activities. Our First Bassoon, Dr. R. L. Spencer, is Dean of the College of Mechanical Engineering of the University of Delaware. He drives 30 miles each way for a rehearsal every Wednesday evening. Our Second Bassoon comes from a distance of 10 miles in the opposite direction. Our leading Doublebass, R. C. Disque, is Dean of Drexel Institute. We have among our organization ten engineers, two chemists, a physicist (not counting myself) two physicians, a dentist, etc. Our chief Cellist, Mr. Natcho Vasileff, is a chemist, and he adds to his accomplishments that of a composer; the Nocturne which we played at the American Association for the Advancement of Science Reception was composed by him and dedicated to me."

F. R. Moulton,
*Permanent Secretary*

## NUTRITION AND GROWTH OF PLANTS[1]

A concept at one time often held by students of plant nutrition was that the absorption of inorganic nutrients by roots is a process in which the roots play a passive role and main emphasis was placed on permeability factors. Some early studies in the California Agricultural Experiment Station on barley plants and later on cells of the fresh-water alga *Nitella*, from which latter vacuolar sap only slightly contaminated could be obtained, gave evidence that ions can be absorbed by plant cells against concentration or activity gradients, with the use of metabolic energy. With this interpretation in view, F. C. Steward conducted experiments of basic importance on potato tuber tissues, and the relation of aerobic metabolism to ion accumulation (movement and storage against a gradient) was demonstrated for these tissues.

Of direct interest to the subject of the paper are the extensive studies of Hoagland and Broyer on the metabolic processes in root cells involved in the accumulation of inorganic solutes and also certain types of movement of these solutes from the roots to the upper parts of the plant. A technique was developed by which young barley plants produced root systems of extremely high capacity to accumulate mobile ions, *e.g.*, potassium, bromide and nitrate ions. In

DR. DENNIS ROBERT HOAGLAND

[1] Review of the paper entitled "Availability of Nutrients for Plant Growth with Special Reference to Physiological Aspects," presented by D. R. Hoagland and D. I. Arnon at the meetings of the American Association for the Advancement of Science, December, 1940.

many of the experiments some of the great complexities of the whole plant system were avoided by making observations on excised roots over brief experimental periods. In this way direct evidence was secured with reference to the effects on salt accumulation of oxygen, carbon dioxide, temperature, concentration of salt and hydrogen ion concentration in the external environment, and of available carbohydrate in the root cells. Briefly, the evidence led to the conclusion that the most rapid absorption of ions and their accumulation against gradients occurred as a result of protoplasmic activities reflected in aerobic respiration for which a supply of oxygen, sugar and presumably of certain growth substances are essential. Sugar and some other required organic constituents are, of course, normally derived from the photosynthesizing tissues of the plant. When other conditions do not limit, the temperature coefficient of the accumulation process is found to be high.

The efficiency of highly active roots in removing from a nutrient solution ions like potassium and nitrate is very high. In fact, concentrations may sometimes be reduced almost to zero in the solution at the same time that the sap from the roots has high concentrations of the ions absorbed. Reciprocal relations of root and shoot in the process of nutrient intake by the plant may be more clearly envisaged on the basis of these and other researches along similar lines. The remarkably rapid and selective absorption of certain ions from very dilute solutions is of great interest in the study of availability of nutrients in soils. Experiments by Arnon and his associates using other types of technique also developed evidence of the importance of metabolic relations in the study of problems of general plant nutrition, including researches on nitrate and ammonium salts as sources of nitrogen in the culture medium.

Since there is naturally a limit to the salt-holding capacity of root cells, the absorption of ions by intact plants over any extended period depends not only on the intake factors already described but also on the upward movement or transport of solutes to the shoot. Experiments over limited intervals of time on young barley plants with very active root systems showed that this movement can take place about as readily in the dark as in the light. Excellent growth of such young plants was possible even when nutrients were supplied only during night periods. Absorption of nutrients was approximately the same for 12-hour periods including either day or night. In other words, the plants were at work 24 hours a day when nutrients were made continuously available and the metabolic activities of the roots maintained. However, root pressure conditions are important in these cases, when transpiration is reduced. With older and larger plants the evidence is that continued absorption of ions may depend, indirectly, on transpiration as a

DR. D. I. ARNON

WATER CULTURE EQUIPMENT

FOR STUDY OF NUTRITION OF LETTUCE PLANTS WITH REFERENCE TO AERATION OF ROOTS AND PROPERTIES OF NUTRIENT SOLUTIONS.

YOUNG BARLEY PLANTS

GROWN BY TECHNIQUE ADAPTED TO PRODUCTION OF ROOTS OF HIGH CAPACITY TO ABSORB AND ACCUMULATE CERTAIN IONS.

means of accelerating upward movement of solutes. But even in transpiring plants some experiments suggest that cell activities are involved in the movement of solutes into the conducting tissues. The writers of the paper, while stressing the significance of active absorption for normal plant growth, also call attention to the possibility that under the influence of transpiration, another type of absorption and upward movement of solutes may take place through inactive or injured roots, and this is illustrated by experiments in which the culture solutions contained sodium salts in increasing concentration up to a point of injury.

In investigations of the absorption of ions and their movement in the plant it is frequently desirable to study established plants over short periods during which the plant system as a whole undergoes as little alteration as possible. The problem then may arise as to technique by which small amounts of elements absorbed and distributed in the plant during the experimental period may be determined. Special difficulties must be faced when it is decided to study the same elements that are already present in the plant at the beginning of the experiment. Fortunately these difficulties can often be surmounted by the aid of the new tool of radioactive isotopes, and various workers in the laboratory have employed radioactive potassium, phosphorus, sodium, bromine and rubidium in the further investigation of problems of absorption and movement of inorganic solutes in the plant.

Jenny and Overstreet in this laboratory have found great value in the radioactive isotopes as a means of study in their attack on the problem of contact intake of ions by plant roots. Evidence was obtained that plants may absorb some ions by direct contact exchange of ions between roots and soil colloids as well as from the soil solution. The general technique already referred to for

making observations on actively absorbing roots was utilized in the researches on absorption of ions from suspensions of soil colloids.

Accumulation of solutes may occur, not only in root cells, but likewise in other parts of the plant. Arnon and Stout carried through a series of experiments on fruiting tomato plants in which they followed the movement of radioactive phosphate added to the nutrient solution when the plants had reached the desired stage of development. The younger and most rapidly growing fruits had the greatest ability to accumulate the newly supplied phosphate. Its distribution in fruit or foliage was demonstrated in graphic manner by making radiographs of the tissues. By employing radioactive isotopes, Stout, Hoagland and Broyer, and Bennett have gained an especially definite kind of evidence on the old problem of which plant tissues are mainly responsible for the upward movement of salts and of the relation of transpiration to this movement.

The authors devote part of their paper to a discussion of the general significance of physiological investigations for the practical problems of plant nutrition. They cite their experiments to compare the growth of plants in soil, sand and water culture media as bearing on the importance of effects of aeration of the root medium. It is pointed out also that aside from direct effects of oxygen on the process of ion absorption, aeration of the soil may profoundly influence the growth of roots and the development of root surface. Under favorable conditions this surface may be enormous in extent and thus innumerable contacts established with soil particles. This exploration of soil by roots and all the factors that influence it, including the internal metabolism of the plant and the climatic conditions, as well as physical and chemical

CHAMBER FOR CONTROLLED GROWING CONDITIONS

PLANTS UNDER CONTROLLED CONDITIONS OF LIGHT, TEMPERATURE, HUMIDITY AND NUTRIENT SOLUTION, IN SOME OF THE STUDIES ON ABSORPTION AND TRANSPORT OF NUTRIENT IONS. THIS ILLUSTRATION SHOWS TOMATO PLANTS USED IN A PRELIMINARY EXPERIMENT OF ANOTHER TYPE.

conditions in the soil, must be of great consequence for an understanding of availability of nutrients.

Specific questions of potassium and phosphate absorption by plants under different soil conditions are discussed from the physiological point of view. Some attention is also given to the micro nutrient elements (elements effective in plant growth in very minute amounts). The climatic influence affecting the requirement or absorption of zinc by plants is cited as an example of a physiological interrelation of soil and plant.

This paper was written as a contribution to a symposium dealing with the general subject of absorption of nutrient ions by plants and their availability in soils for plant growth. The field was broad and the writers considered not only their own investigations, but also those of other workers in the University of California Laboratory of Plant Nutrition who have dealt in recent years with physiological aspects of the general problem under review. The illuminating results obtained by Professor Hoagland and other workers in the laboratory have demonstrated that the plant correlates a multiplicity of functions to acquire from the environment the necessary nutrients and to distribute them to the various centers of activity.

H. S. REED

## RAYMOND PEARL, 1879–1940[1]

IN the death of Raymond Pearl on November 17, 1940, biology loses one of its outstanding figures. He was born at Farmington, New Hampshire, on June 3, 1879. At Dartmouth College he studied biology under William Patten and H. S. Jennings. On his graduation in 1899 he was appointed assistant in zoology at the University of Michigan, where he received the degree of doctor of philosophy in 1902.

In his memorial of Karl Pearson, Pearl has told how Pearson's "The chances of death" stirred his undergraduate imagination and enthusiasm. "It was alive, hearty, vigorous. It was about a lot of things you could do something about. It inspired curiosity and action, rather than awe. To a callow budding biologist, very young and very ignorant, it opened enchanting vistas of possibilities in biological thinking and research before undreamed of." It is little wonder then that the years 1905 and 1906 found Pearl a student in Pearson's biometric laboratory at University College, London. Although the two did not always agree on biologic matters, Pearson's influence on Pearl was strong.

[1] Submitted for publication on December 30, 1940.

From 1907 to 1918 Pearl was head of the department of biology of the Maine Agricultural Experiment Station. During this period his work dealt largely with the biology of the domestic animals, notable researches being on the inheritance of egg production and of milk production.

On the entry of the United States into the First World War Pearl became chief of the statistical division of the United States Food Administration. His studies on the relation of food supply to population are presented in "The nation's food."

From 1918 until his death Pearl was on the faculty of the Johns Hopkins University, first as professor of biometry and vital statistics in the School of Hygiene and Public Health, then from 1925 to 1930 as director of the Institute for Biological Research, and finally as professor of biology in the School of Hygiene. Although much of his attention was given to research, his influence as a teacher was felt by many of the younger generation of statisticians. Most of Pearl's studies at Johns Hopkins centered around the biology of population growth and of the factors that enter into it, such as birth rates and death

RAYMOND PEARL

rates. In 1798 Malthus had emphasized the checks to the growth of population, but the rapid expansion of industry during the nineteenth century had led most students of the subject to disregard them. Pearl was, so far as we know, the first writer of recent times to re-emphasize that there must be a finite upper limit to any population. This was one of the postulates from which Pearl and Reed derived the logistic curve, which, it was later found, had been proposed nearly three quarters of a century before by the Belgian mathematician, Verhulst. This curve fits closely to the growth of the populations of a large number of countries. It also describes the growth of experimental populations of fruit-flies.

A related subject which Pearl also investigated by the experimental method was the inheritance of the duration of life. Different lines of descent of fruit-flies, he found, had different distributions of longevity and when a fly of a long-lived line was mated with one of a short-lived line the duration of life of their descendants followed the ordinary course of mendelian inheritance. Naturally one can not investigate the inheritance of longevity in man by the experimental method, but it has been found by the statistical method that in man, as in the fruit-fly, inheritance is an important factor in determining the duration of life.

Connected with these investigations of the factors that affect death were the investigations of the factors that affect birth described in "The natural history of population." These included the analysis, not only of official vital statistics on birth rates, but also of information about the reproductive histories of a large sample of women, obtained from them while they were patients in the obstetric services of hospitals.

Besides these researches of his own in the field that unites biology to the social sciences, Pearl found time to aid in the advancement of science both by participation in the direction of scientific societies and by the editing of scientific journals. He was the leading spirit in the formation of the International Union for the Scientific Investigation of Population Problems, of which he was president from 1928 to 1930. He was also at various times president of the American Association of Physical Anthropologists, the American Society of Zoologists, the American Society of Naturalists and the American Statistical Association and a member of the council of the National Academy of Sciences. He was the founder and the editor until his death of *The Quarterly Review of Biology* and of *Human Biology*.

Pearl once commented on the great similarity between original creative effort in art and in science. The artistic side of his own nature is shown, not only by his delight in music, but also by his literary skill in presenting his scientific results. Certainly no one could apply to his writings his criticism that "scientific journals are, at the best, occasionally dull, and, at the worst, always so."

Another token of his artistic sensibility was his care in the planning of headpieces and tailpieces for *The Quarterly Review of Biology* and *Human Biology*. The bowman who forms the leitmotiv of the latter was taken from the wall decoration of a paleolithic rock shelter. The prehistoric artist, who was born before the days of Queen Victoria, had represented the bowman with all his members. When the design was submitted to the publisher of *Human Biology*, he protested that it would offend the postal authorities. The bowman was therefore emasculated and the putative sensibilities of the postal authorities were spared.

John R. Miner
Joseph Berkson

## NEW "INDEX EXHIBIT" AT THE SMITHSONIAN INSTITUTION

On Monday, January 20, after six months behind closed doors, the Smithsonian Institution in Washington opened to the public a unique and fascinating exhibit designed to clarify for its millions of visitors the diverse activities and affiliations of the institution. For 95 years the Smithsonian in its laboratories and study rooms has delved into the mysteries of many branches of science; hundreds of its expeditions have gone out to the far corners of the earth in search of new facts and materials; its investigations have been recorded on nearly 500,000 pages of print, making up thousands of volumes of basic scientific knowledge that can be found in most of the world's large libraries. As the years have rolled by and our country has grown in size and grandeur, so too has the Smithsonian grown and expanded its sphere of usefulness. Its fields of activity have multiplied and its physical equipment of buildings for research and exhibition have increased in number.

For the benefit of visitors who are puzzled by the apparent heterogeneity of the institution's activities, the new exhibit is planned so as to classify these activities under major headings, briefly defined. The great Gothic hall of the institution, 123 feet long by 50 feet wide, is divided into 12 alcoves, each with its appropriate heading. Each has a central theme, intended to visualize in some striking way the significance of the particular activity portrayed.

As the visitor enters the hall, he sees on the right a quadrant whereon are listed the subjects of Smithsonian activities in the order in which they are exhibited; also, a list of the eight methods used by the institution for the diffusion of knowledge. On this first quadrant is a large sign telling him that to see the

ONE CORNER OF THE EXHIBITION HALL AT THE SMITHSONIAN INSTITUTION
SHOWING SECTIONS OF THREE OF THE EXHIBITS.

SMITHSONIAN INSTITUTION BUILDING

WHERE THE NEW EXHIBITION IS LOCATED.

exhibit in logical order he should begin with the adjoining alcove and proceed completely around the hall.

The first exhibit is that of astronomy, and this is followed in turn around the hall by geology, biology, radiation and organisms, National Zoological Park, history, physical anthropology, cultural anthropology, engineering and industries, and art. The final quadrant—the last thing the visitor will see as he leaves the hall—is devoted to a pictorial explanation of the organization and branches of the Smithsonian, so that the visitor will leave with an understanding of the present rather complex set-up of the institution.

A separate room is devoted to exemplifying the Smithsonian's methods of diffusing knowledge, the second major objective of the institution.

The theme of the entire exhibit is simplicity. A multiplicity of objects is carefully avoided, and all labels are brief and plainly worded. The strictly enforced aim is to give a quick, easily comprehended bird's-eye view of Smithsonian activities and organization.

WEBSTER TRUE,
*Editor*